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Phil.ⁿ Earl Stanhope.

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

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PHILOSOPHICAL

TRANSACTIONS.

VOL. XLVIII. PART I.

PHILOSOPHICAL TRANSACTIONS.

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Page 201, for *Art.* XXXII. *read* XXXI. P. 34, *Note* (1) *for* TAB. I. *read* TAB. II. In Sheet (Y) *for* 173, *read* 169. *For* 176, *read* 172. *For* 177, *read* 173. *And for* 180, *read* 176.

Page 333, *Note* (2) *for* TAB. III. *read* TAB. II.

I. *An*

I. *An Account of an extraordinary Stream of* Wind, *which shot thro' Part of the Parishes of* Termonomungan *and* Urney, *in the County of* Tyrone, *on* Wednesday October 11, 1752. *by* Wm. Henry, *D. D. Rector of the Parish of* Urney: *Communicated by the right honourable the Lord* Cadogan, *F. R. S.*

Read Jan. 11, 1753.

THE air for the whole day was serene and calm; sometimes a gentle breeze from the south-east. About four of the clock in the afternoon, the sky seemed to open; and there was a flash of lightning from the south-east. In the space of half an hour after, there was heard thunder, as at a great distance, from the same point. About five the sky was a little overcast with clouds; but the air continued in a dead calm. On a sudden there was heard a violent rushing noise; the sky seemed to open, and emitted a flash of lightning, but no noise of thunder; and a stream of wind instantly ensued, the violence of which nothing could resist.

This stream of wind, so far as can be traced by the effects, arose from a glin. called Allgolan, and continued its course for three miles from south-east to north-west. The violent current of it seemed to be confined to a space about 16 feet in breadth, and the whole body of the air in motion did not exceed sixty feet, as may be computed from some of the following particulars, which happened in the little

village of Liſnacloon in the pariſh of Termonomungan, and the edge of the pariſh of Urney.

At the diſtance of a mile to the ſouth-eaſt of this village, it cut a line thro' ſeveral clamps of turf, which were ſtanding in a bog, and tumbled down all the clamps in this line. Thence it croſs'd the river Derge, in the ſame line, and daſh'd up the water with great noiſe and violence, as was obſerved by John Kyle, who has mills on the river, and ſeveral others. Thence, in the ſame line, and at the ſpace of half a mile, it took the village of Liſnacloon, where there are 13 dwelling-houſes, beſide office-houſes, belonging to farmers and cottagers, ſcattered irregularly.

1ſt, It daſhed down an hay-ſtack belonging to Wm. Montieth, which was the firſt object in its way; and ſtripped intirely twelve feet off the roof of his dwelling-houſe.

2dly, It knocked down Henry Carolan's turf-ſtack, and carried ſome of the turf above 300 yards over the cabbins into the fields.

3dly, At the diſtance of 69 paces, it took Henry Crawford's houſe. Full in the broadſide of which, it ſtripped 59 feet, leaving each of the ends, above and below the ſtream of air, quite unmoved. This particular points out its utmoſt breadth.

At the back of this houſe it overſet an hay-rick, which ſtood in its line; but did not ruffle any of the corn-ſtacks, which ſtood within a few yards to the north ſide.

4. It knocked off eight feet of the roof of Solomon Folliot's kiln, which ſtood in its line.

5. It levelled 55 feet of David Montieth's garden-ditch.

6. It levelled, in the ſame direct line, Wm. Folliot the younger's hay-ſtack, which ſtood ſouth-eaſt from his houſe.

7. It burſt with incredible violence thro' his cow-houſe, and cut a paſſage of 16 feet quite thro' it, and carried ſome of the ribs of the houſe before it 400 yards into the field: The reſt of the houſe was a little ruffled. His wife, who was gone into the cow-houſe a minute before, was knocked down by one of the ribs falling. She declared, that it was a dead calm the minute before; when, on a ſudden, ſhe ſaw a flaſh of lightning, and heard and felt the violent ſtorm; but heard no thunder.

Old William Folliot, aged 93, who was walking in the field, at the back of the houſe, was blown down, and grievouſly bruiſed. He ſaw the lightning, but heard no thunder.

Solomon Folliot, being in the ſame field (but out of the line, in which the ſtream of wind paſſed) felt no wind, but heard a mighty ruſhing noiſe, and ſaw the timber, thatch, turf, and duſt of the houſes, fly by him, at the diſtance of forty yards. He ſaw a flight of rooks daſhed down in the ſame field.

In this village are ſeveral other inhabited houſes, both on the north and ſouth ſides of the courſe of this ſtream, none of which were in the leaſt ruffled. The air continued ſtill, among theſe houſes; and the inhabitants ſtood aſtoniſhed, on ſeeing the ſudden devaſtation ſo near them.

After paſſing this village, the ſtream was continued in the ſame line, but with leſs violence, to a

large hill in the pariſh of Urney, which is called Muckle; and, on the north ſide of the hill, at the diſtance of a mile from Liſnacloon, burſt open the door of John Ranking, a weaver, and broke down a web in his loom. As at this laſt place it entered a large bog, which is extended for three miles, it could be traced no farther.

The time, in which this ſtream paſſed thro' the village of Liſnacloon, was about five minutes. It was ſucceeded immediately by a torrent of rain.

Having been informed of this extraordinary phænomenon, that I might have the more perfect knowlege of all particulars, I took with me two gentlemen, Dr. Michael Law, a phyſician of note, and the reverend Charles Rhea, on the 20th inſtant, from Strabane, and view'd and meaſur'd on the ſpot the courſe of this violent ſtream, as it appeared by the marks; and at the ſame time examined minutely the ſeveral inhabitants of the village of Liſnacloon, who were eye-witneſſes of this fact; and from their united teſtimonies, and my own ocular obſervations, collected the above account.

Given under my hand this 21 day of October, 1752.

William Henry, *D. D.*
Rector of the Pariſh of Urney.

II. *An Account of a Book, intitled*, P. D. Pauli Frisii Mediolanensis, *&c.* Disquisitio mathematica in causam physicam figuræ et magnitudinis Telluris nostræ; *printed at* Milan *in* 1752. *inscribed to the Count* de Sylva, *and consisting of Ten Sheets and a half in Quarto: By Mr.* J. Short, *F. R. S.*

Read Jan. 18, 1753.

1. IT may be laid down as a rule in mix'd mathematics, "That the determination of no physical quantity be carried farther than the observations, or other mechanical "measures, can bear;" lest there follow this incongruity, of the conclusion being more extensive than the premises. It were absurd, for instance, in the resolution of a triangle, to compute an angle to the exactness of seconds, or a side to centesms of an inch, when, perhaps, the instruments used can measure no angle less than 10 minutes, or a side but to the exactness of a foot. The conclusions of arithmetic and geometry are indeed rigorously true, but they are only hypothetical; and whenever the quantities, that enter any practical question, cannot be measured, but within certain limits, it were in vain to look for an answer perfectly accurate. The error of the instrument becomes itself one of the *data*; and we must content ourselves to find the limits, which the quantity sought cannot well exceed, or fall short of, by such rules, as the great Mr. Cotes has left us in his excellent treatise on the subject.

2. In

2. In like manner, when any phyfical theory is deduced from obfervations, its accuracy will ftill be in proportion to that of the obfervations, on which it is founded. Sir Ifaac Newton, we find, in computing the ratio of the earth's axis to its equatorial diameter, confines himfelf to a reafonable approximation, and to three places of figures (229 to 230); becaufe, whether that ratio is deduced from the different lengths of ifochronous pendulums in different latitudes, from the meafurement of diftant degrees of a meridian, or from both; the elements of the calçulus can fcarcely furnifh a greater degree of exactnefs. And of the fame judicious caution we have many other examples in the works of that incomparable author.

3. On the other hand, when obfervations and theories are brought together, and compared, nothing can be juftly inferred againft a theory from its difagreement with the obfervations, unlefs that difagreement is greater, than can be fairly imputed to the imperfection of inftruments, and to the unavoidable miftakes of an obferver; efpecially, if the difference fhould be fometimes in excefs, and at other times in defect; or, according to fome of the obfervations, fhould intirely vanifh.

4. Altho' thefe rules, manifeftly well-founded, have been followed by all the beft writers, our author obferves, that feveral ingenious men, both in France, and in Italy, have deviated from them; particularly in treating of the famous queftion concerning the figure of the earth. Some, with Meffieurs Clairaut and Bouguer, attributing too much to the obfervations, that have been made, and taking them

for

for abſolutely exact, have concluded Sir Iſaac Newton's reaſonings on that ſubject to be faulty; while Father Boſcowic, a Jeſuit at Rome, making them quite looſe and uncertain, thinks no argument at all can be drawn from them, concerning the earth's figure; far leſs in confirmation of the Newtonian theory.

5. In oppoſition to theſe two extremes, equally contrary to reaſon, as they are to each other, F. Friſi writes the treatiſe now before us; in the introduction to which he ſhews, 1. That, altho' the ratio of the axis of the earth to its equatorial diameter is, from M. de Maupertuis's operations in Lapland, and afterwards in France, that of 177 to 178; and by the theory only 229 to 230; yet the difference is no more, than what might ariſe from a miſtake of about 60 toiſes in the meaſure of either of the two degrees, that are compared, or of 30 toiſes in each of them. Or, ſuppoſe the meaſure of the arcs to be exact, the ſame difference might be owing to an error of 4 or 5 ſeconds in the aſtronomical part. And ſuch errors, or others equivalent to them, in a courſe of ſo many combined operations, our author looks upon as difficult to be avoided. But he adds, if the obſervations of M. de Maupertuis, and his fellow academicians, ſeem to differ from the theory, thoſe of Meſſieurs Bouguer and de la Condamine exactly agree with it: According to whom, a degree at the equator, containing 56753 toiſes, and in latitude 49° 22′ 57183 toiſes, the difference of the axis and equatorial diameter comes out to be $\frac{1}{229}$.

6. In anſwer to F. Boſcowic, and thoſe who make no account of the obſervations, our author allows, that,

that, if they were ſuch, as M. Caſſini, and ſome other academicians, made in France, of the meaſure of a parallel of latitude, they could not be much depended on; that method being liable to ſeveral obvious inconveniencies. But he inſiſts, that, with the excellent inſtruments, which were uſed, and, conſidering the diſtinguiſhed ſkill of the obſervers, as well at the polar circle as in France, and at the equator, the error upon one degree of the meridian could not exceed 60 or 70 toiſes; which is a degree of exactneſs not only ſufficient for the determination of the firſt queſtion, *viz.* whether the ſpheroid of the earth is flat or long; but likewiſe to found an agreement between the obſervations and the theory, as near as can be expected or deſired.

7. The work itſelf is divided into ten chapters:

I. De obſervationibus circa telluris figuram hactenus inſtitutis.
II. De principiis et hypotheſibus quibuſdam.
III. De rotatione corporum, et vi centrifuga.
IV. De mutationibus ex motu circulari ortis.
V. De attractione corporum rotundorum.
VI. De comparatione gravitatis in variis homogeneæ ſphæroidis locis.
VII. De figura terræ.
VIII. De gradibus meridiani et parallelorum.
IX. De loxodromiis nautarum, de parallaxi lunæ, et aliis ex eadem theoria pendentibus.
X. De theoriæ et obſervationum conſenſu.

8. In chap. I. we have a ſhort hiſtory of the inquiries, that have been made into the magnitude and figure

figure of the earth down to the preſent times; and the preference is juſtly given to the meaſurements of Mr. Norwood in England, A. D. 1635, and of the members of the French Academy of Sciences ſince that time. From theſe he gathers, that, within leſs than 60 or 70 toiſes, the lengths of a degree of the meridian are as follows:

	°	′	Toiſes.
Lat.	0	0	56753
	45	0	57100
	49	22	57183
	53	0	57300
	66	20	57400

9. Chap. II. contains an account of the principles, on which this theory is founded; *viz.* the univerſal gravitation of matter, and the diurnal rotation of the earth. Our author mentions likewiſe the hypotheſis of the earth's being originally in a fluid ſtate; but rejects it as precarious and improbable. He allows however, that, with regard to the preſent queſtion, it is all one whether it was firſt a fluid or not, ſeeing the ocean is circumfuſed juſt in the ſame manner, and to the ſame altitude, as if the whole was ſtill a fluid.

10. Chap. III. and IV. are employed in the doctrine of centrifugal forces, and their effect in changing a fluid ſphere into the form of an oblate ſpheroid. In the former of theſe chapters, the author reſolves, as uſual, the centrifugal force of a particle into two others; one, that acts directly contrary to the gravitation of the particle; and the other a force in a direction perpendicular to it. And this laſt he conſiders

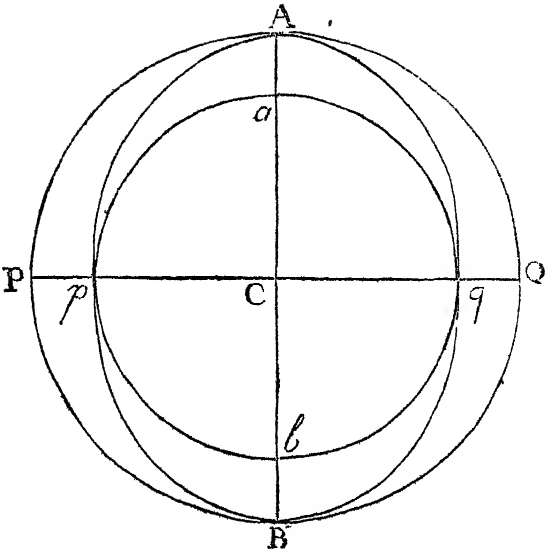
again as acting laterally upon the contiguous particles impelling them towards the equator. But the quantity of this force, when greateſt at the octant, he computes to be only $\frac{1}{668168}$ of the force of gravity; and therefore, ſays he, it may be ſafely neglected. The truth is, after the ſpheroid is come to be in a permanent ſtate, and all its parts in *æquilibrio*, there is no longer any ſuch lateral force at all; it being now intirely ſatisfied by the gradual contraction of the earth's axis.

11. The general contents of the following chapters are ſufficiently expreſſed in their titles already given. Nor can we be more particular, without entering into a detail of algebraical operations, which would be improper for this place; and which is the leſs neceſſary, as the ſame things have been treated of by ſeveral other authors.

This does not, however, in the leaſt detract from the merit of F. Friſi; who diſcovers throughout this work much acuteneſs and ſkill, joined with all the candor and ingenuity, that become a philoſopher. And as he has not yet exceeded his 23d year, it may be expected, that the ſciences will one day be greatly indebted to him; eſpecially as we find him actually engaged in compoſing a complete body of phyſico-mathematical learning.

12. There is only, in his VI. Chapter, a criticiſm upon one of Sir Iſaac Newton's demonſtrations, in which we cannot agree with this ingenious author. And as this demonſtration has proved a ſtumbling-block, not only to F. Friſi, but to many other learned men, we ſhall be obliged to conſider that part of it, which has been miſtaken, at ſome length, by the help of the following ſcheme:

In



In which let the ellipsis $ApBqA$, whose axes AB, pq, are in any given ratio, as of m to n, have the circles $apbq$, and $APBQ$, inscribed and circumscribed to it: And if the figure revolves on the axis PQ, there will be generated an oblate spheroid $ApBqA$, with two spheres, the greater circumscribed to the spheroid, and touching it in its equator APA, and the lesser inscribed and touching it in the poles pq; the solid content of the spheroid being the first of the two mean proportionals between the solidity of the exterior sphere, and that of the interior.

But if the figure revolves on the axis AB, there will be generated a prolate spheroid $ApBqA$, inscribed in the exterior sphere at the poles AB; and circumscribing the interior sphere at the equator pqp, its solidity being the second of the above mean proportion-

 als.

als. So that if O and P ſtand for the ſolidities of the oblate and prolate ſpheroids, and S, s, for the two ſpheres; $S : O : P : s$ ÷ are in the continued proportion of $m : n$. And $S : P$, or $O : s :: m^2 : n^2$. As $S : s :: m^3 : n^3$.

Or we may with Sir Iſaac Newton conceive of the geneſis of theſe ſolids as follows. 1. Let the ſphere $APBQ$ be uniformly compreſſed in the direction of its axis PQ, till that axis is diminiſhed to pq, and the ſphere changed into the oblate ſpheroid. 2. Let this ſpheroid be equally compreſſed in the direction of that diameter of its equator, which is perpendicular to pq and AB, or to the plane of the figure; and it will degenerate into the prolate ſpheroid, whoſe poles are A and B. 3. Let this laſt be compreſſed in the direction of its axis AB, till it is changed into the ſphere $apbq$; and, in each of theſe compreſſions, the ſolid ſpace, which the body contains will be diminiſhed in the ratio of m to n.

Now, as the determination of the earth's figure depends not only upon that of the ratio of the centrifugal force, by which a body tends to recede from the axis of rotation to the power of gravity; but likewiſe, upon the decrement of gravitation ariſing from the body's being in that rotation actually removed to a greater diſtance from the centre: it is not enough, that we know, from the experiments with pendulums, the centrifugal force at the equator to be about $\frac{1}{289}$ of the force of gravity. We need, farther, two diſtinct propoſitions; one to determine the attractive force of a ſpheroid at its pole; and the other to determine its attraction at the equator. The firſt of theſe we have in *Princip. lib.* 1. *prop.*

91. and the ſecond has been ſupplied by ſeveral authors. But Sir Iſaac, who ſeldom does any thing in vain, found, that he could, by one of his artifices, make that 91ſt propoſition ſerve likewiſe to determine the attraction at the equator, by the following argument:

Let G be the attraction of the exterior ſphere at A; and let the decrement of that attraction, when the ſphere is diminiſhed into the oblate ſpheroid $ApBq$, be d; and δ the decrement of this laſt attraction, when the oblate ſpheroid is diminiſhed into the prolate, whoſe poles are AB: then, I ſay, d is nearly equal to δ; the difference of the axes of the generating ellipſe being ſmall.

For the attractive matter, that is taken away, has, in both caſes, the ſame ratio to the matter, that is left; and its poſition, with reſpect to that which is left, is, in both caſes, nearly the ſame: And therefore the ſucceſſive attractions will be nearly in continued proportion, $G : G - d :: G - \overline{d + \delta} \div$. Or multiplying and rejecting d^2 as inconſiderable, $Gd = G\delta$, and $d = \delta$.

Thus, if the attractions of the ſphere $APBQ$, and of the prolate ſpheroid, at its pole A, be 126 and 125 reſpectively; the attraction of the intermediate oblate ſpheroid at its equator will be $125\frac{1}{2}$: and how nearly this approaches to the truth, may be ſeen from an exact computation of thoſe attractions. For, if the axes of the generating ellipſe be 101 and 100, and the attractive force at the ſurface of the ſphere 126; the attraction at the pole of the prolate ſpheroid will be 124.9838; and that at the equator of the oblate 125.5077; which exceeds the arithmetical mean between

between the two former, only by .0068; that is, by about $\frac{1}{18410}$th part of the attraction of the oblate spheroid at the equator.

This reasoning is more shortly expressed (*Princip. lib.* iii. *Prop.* 19.) as follows:

.... "Gravitas in loco *A* in sphæroidem, con-
"volutione ellipseos (*ApBq*) circa axem *AB* de-
"scriptam, est ad gravitatem in eodem loco *A* in
"sphæram centro *C* radio *AC* descriptam, ut 125
"ad 126. Est autem gravitas in loco *A* in terram
"media proportionalis inter gravitates in dictam
"sphæroidem et sphæram; propterea quod sphæra,
"diminuendo diametrum *PQ* in ratione 101 ad 100,
"vertitur in figuram terræ; et hæc figura, diminu-
"endo in eadem ratione diametrum tertiam, quæ dia-
"metris *AP*, *PQ* perpendicularis est, vertitur in dic-
"tam sphaeroidem; et gravitas in *A*, in *utroque casu*,
"diminuitur in *eadem ratione* quam proxime."

In which the expression "*eadem ratione*" occurring a second time has misled F. Frisi and others, to think this last ratio to be likewise that of the axes, or of 101 to 100: Whereas the *identity* of ratio's here asserted is to be referred only to the words "*utroque casu*;" the ratio itself being not that of the axes, or of *m* to *n*; but the half of that ratio (whatever it is found to be by *Prop.* 91. *lib.* i.) which the attraction of the sphere has to the polar attraction of the inscribed spheroid.

This inadvertence, however, of his own F. Frisi charges upon Sir Isaac Newton; and files it up, as the sixth of the errors, which he says have been discovered in the *Principia*. ... "Ita dum stabilitæ "in 19 *lib.* iii. propositione terrestrium axium pro-

"portionis

"portionis fulcimentum et patrocinium quærimus, "aliud in propositione eadem *sophisma* sese offert, "quod eorum, quæ in principiis mathematicis New-"toni nacta (*i. e.* detecta) sunt hactenus, sextum "est, &c." But we may take it off the file again; and, for the present, leave the other five, till they are considered of at more leisure.

13. In his 10th and last chapter, our author sums up the evidence, and finds, that all the good observations, that have been made, as well by pendulums, as by actual mensuration, concur with the theory, in making the ratio of the earth's axis and equatorial diameter to be as 229 to 230. This is indeed a sufficient confirmation of the theory of gravitation: But it must be observed, that the coincidence is not perhaps quite so perfect as F. Frisi imagines. That ratio corresponds well enough to the exactness, to which the first elements of the calculus can be obtained; *the length of a second pendulum, and that of the earth's equatorial diameter*, from which the centrifugal force ($\frac{1}{289}$) is deduced. But, if we suppose that force to be accurately $\frac{1}{289}$, and compute more rigorously, we shall find the ratio in question to be very nearly that of 225 to 226; agreeing still with the observations as well as can be desired; and shewing, at the same time, the inimitable art of Sir Isaac Newton in the contrivance and use of approximations; seeing the strictest calculation raises the equator not the third part of a mean geographical mile above what he had found by his method.

I sent F. Frisi's book to my ingenious and learned friend the reverend Mr. Murdock, Fellow of this Society, who has fully consider'd the question concerning

cerning the figure of the earth; and who, after having perused the book, and discover'd the above mistake of F. Frisi, sent me the above theorem, and its demonstration. He likewise sent me the following theorems, which, he says, he had communicated to M. de Bremond, in the year 1740, when he was translating his treatise on sailing: But M. de Bremond dying soon after, those, who had the care of publishing the translation, printed it incorrect in several places; particularly the theorems for the prolate spheroid: On which account, he says, if they are thought worth preserving, they may be inserted in the *Philosophical Transactions*.

Postscript.

Theorems for computing the ratio of the attractive force of a spheroid, at its pole or equator, to that of the inscribed or circumscribed sphere.

1. In an oblate spheroid, the ratio is,

$$\left.\begin{array}{l}\text{Pole}\\[2ex]\text{Equator}\end{array}\right\}\left\{\begin{array}{l}1+\dfrac{1}{m^2-1}-\dfrac{m^2}{(m^2-1)^{\frac{3}{2}}}\times A : \frac{1}{3}\\[2ex]\dfrac{m^2}{(m^2-1)^{\frac{3}{2}}}\times A-\dfrac{1}{m^2-1} \quad : \frac{2}{3}\end{array}\right\}$$

2. In a prolate spheroid, the ratio is,

$$\left.\begin{array}{l}\text{Pole}\\[2ex]\text{Equator}\end{array}\right\}\left\{\begin{array}{l}1-\dfrac{m^2}{m^2-1}+\dfrac{m}{(m^2-1)^{\frac{3}{2}}}\times l : \frac{1}{3}\\[2ex]\dfrac{m^2}{m^2-1}-\dfrac{m}{(m^2-1)^{\frac{3}{2}}}\times l : \frac{2}{3}\end{array}\right\}$$

In

In which $m:1$, as the greater axis of the generating ellipfe is to the leffer. A is a circular arc, to the radius 1, whofe tangent is $\sqrt{m^2-1}$, or its reciprocal, if $m^2-1 = 1$. And l is the natural logarithm of $\frac{S}{V}$, S being the fine of the arc, whofe co-fine is $\frac{1}{m} \times \sqrt{m^2-1}$, and V the verfed fine of the fame arc.

Note, The two firft theorems, by fubftituting t for $\sqrt{m^2-1}$, coincide with thofe of Mr. Maclaurin for the oblate fpheroid, in his differtation on the tides.

III. *A Letter from the Rev. Mr.* George Coftard, *Fellow of* Wadham-College, Oxford, *to Dr.* Bevis, *concerning the Year of the Eclipfe foretold by* Thales.

Dear Sir,

Read Jan. 25, 1753.

I THANK you greatly for the ufe of the Peterfburg Acts, while in London, where Bayer fuppofeth the eclipfe foretold by Thales to the Ionians fell out the year before Chrift 603. Since my return home, upon looking over fome papers, that I had formerly drawn up on that fubject, I find, that I had determined it to have been the very fame year. I will not trouble you with the chronological arguments, on which I founded that determination; and therefore fhall only tranfcribe fo much of thofe papers as relates to calculation.

 Riccioli

Riccioli ſuppoſeth, that the eclipſe foretold by Thales happened the year before Chriſt 585; and quotes both Theon and Cleomedes, in confirmation of the opinion. Theon, perhaps, had Cleomedes's words in view; but neither of theſe authors have circumſtances enough to determine what eclipſe in particular they meant. The paſſage of Theon is in his chapter concerning the moon's parallax, where he ſays, that Hipparchus, being in doubt whether the ſun had any parallax at all, ſuppoſed, in the firſt book of his treatiſe concerning Magnitudes and Diſtances, that the earth, in reſpect of the ſun, was only a point; from whence, by means of an eclipſe there ſet down by him, he framed two diſtances of the ſun, a leſs and a greater. *Εν γὰρ τῷ Α περὶ μεγεθῶν ϗ ἀποστημάτων λαμβάνει φαινόμενον τοῦτο, Ἔκλειψιν Ἡλίου, ἐν μὲν τοῖς περὶ τὸν Ἑλλήσποντον τόποις ὅλου τοῦ Ἡλίου ἀκριβῶς γεγενημένην, ὥστε μηδὲν αὐτοῦ παραφαίνεσθαι ἐν Ἀλεξανδρείᾳ δὲ τῇ κατ' Αἴγυπτον τὰ Δ μάλιστα πεμπτημόρια τῆς διαμέτρου ἐκλελοιπότα.*

All then that is here ſaid is, that the eclipſe made uſe of by Hipparchus was total at the Helleſpont; but at Alexandria in Egypt a little more than 5 digits only. But he hath neither given us the æra of Nabonaſſar, the place of the luminaries, nor any one circumſtance beſides, by which we might form any concluſion what year this eclipſe was in.

Cleomedes, who perhaps ſaw the ſame treatiſe of Hipparchus, is as uncircumſtantial as Theon. He ſays only, that the diameter of the moon's ſhadow at the earth is ſomething more than 4000 ſtadia. And to confirm this aſſertion, he ſays, *γεγόνε δὲ ϗ αὐτὴ τήρησις ἐπὶ τῆς κατὰ τὸν Ἥλιον Ἐκλείψεως, ὃς ὅλος ΠΟΤΕ*

ἐν Ἑλλησπόντῳ ἐκλείπων, ἐτηρήθη ἐν Ἀλεξανδρείᾳ παρὰ τὸ πέμπτον τῆς ἰδίας ἐκλείπων διαμέτρου· ὅπερ ἐστὶ κατὰ τὴν φαντασίαν παρὰ δακτύλους δύο καὶ βραχύ.

By the quantity of obscuration here mentioned, this seems to have been the same eclipse with that quoted by Theon from Hipparchus; but as the place of observation in both these authors appears to have been Alexandria in Egypt, it must have been after that place was built. Consequently it was probably observed there by Hipparchus himself, and therefore could not have been the eclipse foretold by Thales. Besides, was this eclipse total upon the banks of the Hellespont, I know not what reason there is for supposing, that the battle between the Lydians and the Medes was fought there. It should rather seem, that the engagement was on the confines of the two kingdoms: consequently in a more southern latitude, and in a longitude more to the east of Alexandria, this eclipse could not have been total; nor therefore (as Herodotus said it did) turn day into night.

Sir Isaac Newton, in his chronology, likewise supposes the eclipse meant to have been that in May, the year before Christ 585. But in this, perhaps, he rather followed others, than adopted it after any examination of his own. That treatise never had the finishing hand of its great author, and it is well known now in what manner it came abroad.

According to Riccioli, this eclipse was at the Hellespont central, and at Sardes fell out at 6 of the clock in the afternoon; and therefore is rejected, you find, by Maier, in the Petersburg Acts, as being too late in the day. " Quia ad littora Asiæ minoris

" (ſays he) ſub ſolis occaſum appulit, in Natolia
" nulla fuit eclipſis totalis, quin nec conſpicua ob
" inſtantem ſolis occaſum."

According to my computation, the apparent time of the true conjunction was at Greenwich, May 28, $4^h\ 35'\ 15''$.

	s	°	′	″
The place of the luminaries	1	29	0	24
Moon's latitude north			20	54
Semidiameter of the earth's diſk			61	14
Semidiameter of the penumbra			32	40

	d	h	′	″
Time of the greateſt obſcuration at Greenwich	28	4	32	33
Beginning of the general eclipſe		2	3	20
End of the general eclipſe		7	1	46
Duration		4	58	26

	°	′
Longitude of Sardes eaſt from London	29	5
Its longitude north	38	10

Time of ſun-ſet there May 28, $7^h\ 6'\ 54''$.

The paſſage of the centre of the penumbra over the earth's diſk was at the following times, as reckoned at Greenwich, thus:

Times

Times			Longitudes			Latitudes		
h	′	″	°	′	″	°	′	″
3	22	33	115	5	10W	22	10	40N
4	02	33	95	24	50	31	30	10
4	32	33	75	52	30	38	49	10
4	35	55	73	34	40	39	31	10
4	43	34	68	8	0	41	2	23
5	02	33	52	22	10	44	11	30
5	32	33	25	20	20	46	36	30

By this, I think, it pretty plainly appears, that the centre of the ſhadow paſſed ſo far from any place, where we can reaſonably ſuppoſe the battle between the Lydians and Medes to have been fought, that this can hardly have been the eclipſe mentioned by Herodotus.

Father Hardouin, in his chronology of the Old Teſtament, rejects this eclipſe, as not happening, he ſays, in the reign of Cyaxares, but in that of Aſtyages; not on the fourth year of the Olympiad, but a month before it began; as falling out too late in the day; the greateſt obſcuration being (at Sardes, I ſuppoſe he means) ſcarce half an hour before ſun-ſet; and not total, or central, or 12.56′ digits, as Riccioli makes it, but almoſt 9. Tho' Pliny therefore ſays this eclipſe was Olymp. XLVIII. 4. & A. V. C. CLX. yet ſix MSS. he obſerves, in the French king's library, have CLXX. and ſo moſt printed copies. He thinks therefore, that, inſtead of CLXX the number ſhould be CLVII. which, he ſays, is Olympiad XLVIII. 4. and the year before Chriſt 597; when there was an eclipſe of the ſun, on Wedneſday July the 9, at 6 of the clock in the morning.

This

This eclipſe Petavius likewiſe prefers; tho' he makes the digits eclipſed only 9.22': Which is ſtrange enough, as it could not have been by any means the cauſe of ſuch a darkneſs, as is deſcribed by Herodotus. But F. Hardouin ſuppoſeth, that this battle was fought upon the banks of the river Halys in Cappadocia, and in latitude north 40°; where, ſays he, this eclipſe muſt have been central and annular.

According to Dr. Halley's tables, the year before Chriſt 597 the apparent time of the true conjunction at Greenwich was July 8d 21h 50' 9"; and

	s	°	'	"
The place of the luminaries	3	9	17	0
Moon's latitude north			33	32
Semidiameter of the earth's diſk			54	38
Semidiameter of the penumbra			30	44

	d	h	'	"
Beginning of the general eclipſe		19	8	16
End of the general eclipſe	9	0	49	2
Duration		5	41	6

	°	'	"
Sun roſe centrally eclipſed, in longitude weſt from Greenwich	83	33	0
And in latitude north	43	26	50
Sun ſet centrally eclipſed, in longitude eaſt from Greenwich	132	37	20
And in latitude north	26	25	20

The place of the centre of the penumbra was at the following times, as reckoned at Greenwich, thus:

Times

Times			Longitudes			Latitudes		
h	′	″	°	′	″	°	′	″
21	46	15	30	21	0 E	62	30	34 N
21	58	39	42	53	30	60	53	0
22	13	39	56	32	40	58	8	20
22	28	39	68	58	0	54	41	40

This eclipſe, therefore, at Sardes, or any-where elſe that we can well ſuppoſe this battle to have been fought, could not have been great enough to turn day into night; and therefore doth not anſwer Herodotus's deſcription.

Archbiſhop Uſher rejects both theſe eclipſes, as inconſiſtent with his chronology; and ſuppoſes that intended to have been A. M. 4113. An. Nab. 147. before Chriſt 601. Olymp. XLIV. 4. Sunday July 20. 3^h 25′. before noon, digits eclipſed 9. But this likewiſe is greatly defective as to quantity.

But tho' this is inſufficient for the purpoſe, yet there was one two years before this, or the year before Chriſt 603, that will be found by good tables intirely ſatisfactory. Petavius, indeed, makes the digits eclipſed only 7.20′; but, according to Dr. Halley's tables, the apparent time of the true conjunction was at Greenwich, May 17^d 20^h 42′ 19″. The place of the luminaries 1ſ. 19° 12′. and the moon's latitude north 25′ 17″.

Semidiameter of the ſun	15 49
Semidiameter of the moon	16 45
Semidiameter of the penumbra	32 34
Semidiameter of the earth's diſk	60 50

Beginning

	h	′	″
Beginning of the central eclipſe	19	13	27
End of the central eclipſe	22	3	47
Duration	2	50	20

	°	′	″
Sun roſe centrally eclipſed, in longitude weſt from Greenwich	43	46	10
And in latitude north	2	54	0
Sun ſet centrally eclipſed, in longitude eaſt from Greenwich	155	56	20
And in latitude north	42	38	10

The place of the centre was at the following times, as reckoned there, thus:

Times			Longitudes				Latitudes			
h	′	″	°	′	″		°	′	″	
19	13	27	43	46	10	W	2	54	0	N
20	8	37	19	19	30	E	31	33	0	
20	23	37	28	28	10	E	35	47	50	
20	42	19	40	29	30	E	40	41	20	
22	3	47	155	56	20	E	42	38	10	

By this it appears, that, if modern maps and geographers may be depended upon, the center of the ſhadow paſſed over the kingdom of Barca and Africa, and croſſed the Mediterranean between Candia and Cyprus, and then over Antiochetta and Erzroum, and a little to the ſouth of Kars.

You will ſee, Sir, how this agrees with what is ſaid in the Peterſburg Acts, pag. 332. which, therefore, I ſhall not tranſcribe. I ſhall only add, that, if any allowance is to be made for the moon's acceleration,

for the moon's acceleration, or any other caufe, the track here given, as you know, will be a little different. As I cannot make feveral ancient eclipfes, that I have tried, fucceed to my mind, without fome fuch fuppofition, I have done the fame with regard to this. What the quantity to be allowed is, I leave to you and others to determine: At prefent I make it 45′; at Mr. Whifton's rate of 1′ in 54 years, or thereabouts. Then

	d	h	′	″
The apparent time of the true conjunction at Greenwich	17	20	42	19
For the moon's acceleration			45	0
Apparent time of the moon's conjunction at Greenwich corrected	17	19	57	19

From hence I find the paffage of the penumbra at the following times as reckon'd at Greenwich, thus:

Times			Longitudes			Latitudes		
h	′	″	°	′	″	°	′	″
19	23	37	30	49	30 E	31	33	0 N
19	38	37	39	58	10	35	47	50
19	57	19	60	30	0	40	41	20

By this table it appears, that the center paffed more to the fouth than the former, and went near Tripoli, Aracta, Nifabin, and Ardbil.

It is much to be wifh'd, that Herodotus had told us where this battle was fought; that, by this means, we might have known, which of thefe two paths to have preferred. However, as he hath not, and there

 is

is nothing in either of them, that is inconfiftent with the hiftory, I conclude, from a number of other circumftances befides, that this really was the eclipfe foretold by Thales. I was not a little pleafed, upon looking into my papers, to find, that Bayer and I agreed fo exactly in the very year, as I was a ftranger to what he had faid upon that fubject, till you brought me that volume of the *Petersburgh Acts*; and as the principles I proceeded upon were fomething different from his. This, however, is a prefumption, that we are right, and confirms my opinion of fome other articles in chronology, wherein I differ from fome very great names. I am,

Dear Sir,

Iflip, May 21, 1752.

Your very faithful and

obliged humble fervant,

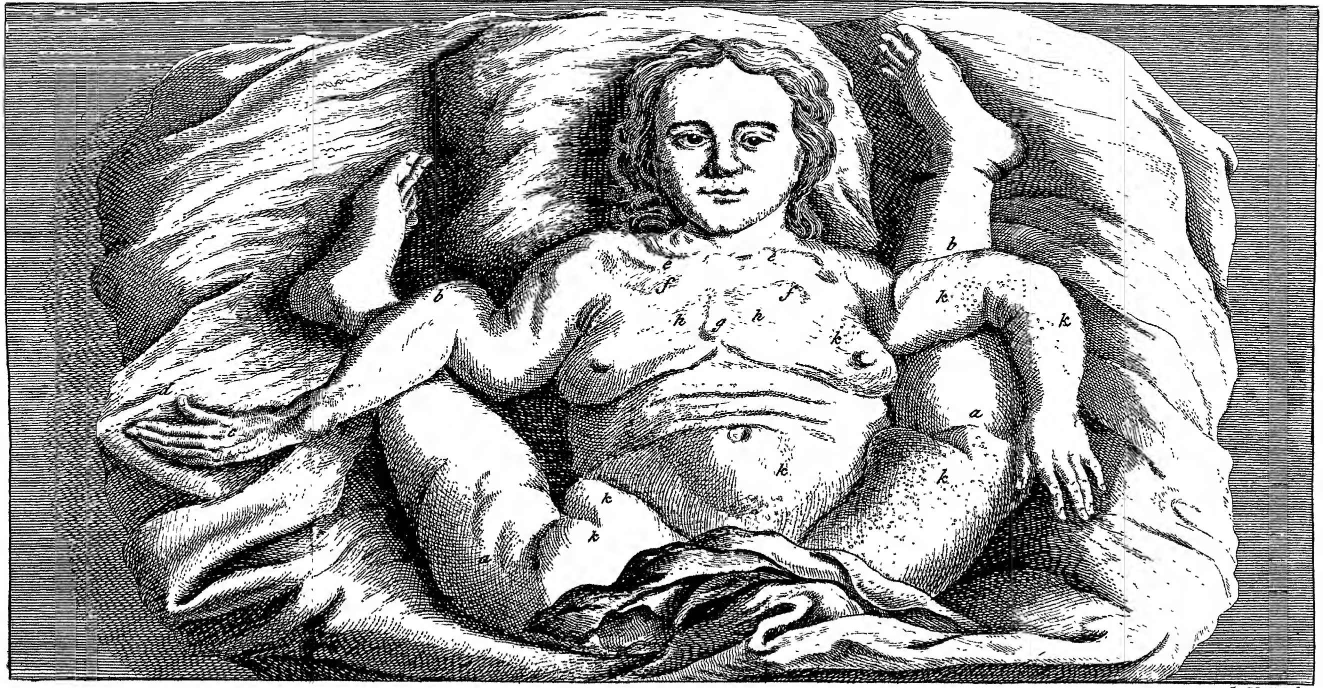
G. Coftard.

IV. *An Account of the Cafe of* Anne Elizabeth Queriot, *of* Paris, *whofe Bones were diftorted and foftened*; *by* Ambrofe Hofty, *M. D. of the Faculty of* Paris.

Read Jan. 25, 1753.

ANNE Elizabeth Queriot *, aged 35, native of Paris, was married in the year 1746 to a wool-carder. Tho' feemingly of

* Her marriage-name was Supiot.



An extraordinary Case of the successive softening of the Bones.

a.a. The Balls of the Knees
b.b. The Condyles or Elbows
c. The right Wrist shrunk in
d. The first Deformity which appear'd
e.e. The Clavicles rais'd into an Arch
f. The point of the Sternam rais'd & turn'd obliquely towards the right
g. Swellings on the second of the Ribs
h.h. The partitions of the Breast depress'd before
i.i. The Toes bent within
k.k. Parts of the Skin become callous.

of a delicate conſtitution, yet ſhe enjoyed tolerable health, having never had any dangerous illneſs before her marriage. She was brought to-bed in 1747, in the month of September, and for the firſt time complained of great weakneſs in the ſmall of her back, loins, and thighs, and could ſcarce walk.

A ſecond lying-in, a year after, removed her complaints for about ſix weeks; after which they returned. In the year 1749, being two months and a half with child, ſhe was unaccountably ſeized with a loſs of blood; upon which ſhe miſcarried.

Two months after, ſhe fell accidentally upon her left ſide; which gave her great pain in the leg, thigh, and hip of that ſide, and made them ſwell: but there was neither fracture or diſlocation.

Her pains in ſome time abated; but the weakneſs of her limbs continued. She was ſoon after with child, and, in the beginning of her pregnancy, had a ſecond fall; which revived her former pains, and cauſed new ones all over her body, and a ſwelling, as before. This confined her to her bed, yet her pregnancy went on very well, and terminated as happily; by which the ſwelling went off; but her limbs were ſo weak, that ſhe could not bear upon her feet.

She then fell into the hands of quacks, who gave nothing worth mentioning, that I could hear of. About a year ſince ſhe was blooded on the hand twice, took a courſe of baths in the decoction of the aſhes of vine-twigs, had different pultices applied to her, and embrocations of oils and balſams, which enabled her to walk by the help of crutches.

In about ſix months after child-birth, her pains returned more violent than before; and about the

ſame time, an abundance of white chalky ſediment appear'd in her urine; and the fore-finger of her right-hand was obſerved to be diſtorted towards the little finger; which was the firſt appearance of the diſſolution, that enſued. Soon after the lower extremities began to turn upwards gradually, and almoſt in a parallel line with her body, and continuing, in nine months was reduced to the poſture exhibited by the print drawn laſt Auguſt. All the bones were affected, eſpecially the thorax, which had loſt its natural form and capacity.

This miſerable ſtate was conſtantly attended with exquiſite pains; and, according to the ſeat thereof, the patient uſed to ſay, "Now ſuch a part works." Sometimes they abated, and then ſhe felt ſo ſore, as not to bear being touched: And during this eaſe from her pains, a quantity of the above-ſaid ſediment paſs'd by urine, and little or none in her ſufferings. It is quite cretaceous, and, reduced into a fine powder, ferments gently with acids. Little of it appeared ſince I began to viſit the patient; ſo that I could not get a ſufficient quantity thereof to make farther inquiry. I refer to the print; it being almoſt impoſſible, without it, to conceive the figure ſhe made, and the diſtortion of her limbs.

She could bear no covering, but a few napkins, both from inward heat, and to avoid loading her breaſt. I ſhall only remark, that, ſince the print was drawn, her right foot and hand got above her head. Notwithſtanding this preternatural poſture, the evacuations by ſtool and urine were regularly performed, and with great eaſe. Her fleſh ſeemed dead and œdematous, the ſkin rough and

ſcaly,

ſcaly; ſo that I often apprehended a mortification. She had a cough, a laborious reſpiration, and ſometimes a ſpitting of blood, from the coarctation of her breaſt, all its bones plying inwardly. She was capable of no other motion but of turning her head on both ſides, ſtirring her left arm in the ſhoulder-joint only, and ſeparating her fingers, but not bending them. She had her menſes regularly, till about three months before her death. She generally had a low fever, inward heat, ſweats, and reſtleſſneſs. She took antiſcorbutics during the months of June and July, to no purpoſe. Her fever ran very high in Auguſt, attended with deliriums, headachs, ravings, and *ſubſultus tendinum.*

A little before her death came on a deafneſs, a dimneſs of ſight, a ſcalding of her eyes, and a conſtant dropping; violent pains in her head; in ſhort, a great weakneſs in all the organs, which ſhew'd how much the head was then affected.

The diſtortion of her limbs went on ſo faſt in Auguſt and September, that almoſt every third day I could obſerve ſomething new; eſpecially the left foot, during that time, came down gradually near 18 inches from under her ear, where it lay before. It was alſo obſerved in Auguſt, that her neck grew viſibly ſmaller, the thorax much narrower. I then alſo remarked, that the napkins, upon which ſhe ſpit, grew black in the waſhing, and ſtained as from the mercurial ointment; tho' I could not ſuſpect it, as I could not learn ſhe had ever uſed any mercury. In a month after, I obſerved the ſame thing on all the linen, that touched her ſkin. I got a napkin rubbed with ſoap, then dried, and afterwards waſhed. This

method

method had almoſt taken off the ſtains, as it does thoſe from the mercurial ointment.

Her linen ſtained all the waſhing, like linen impregnated with the above-ſaid unguent. Thoſe ſpots appeared on the linen a mixture of a cretaceous matter and greaſe.

Since this remark was made, none of the white ſediment was ſeen. This, and the apparent nature of the ſtains, made me believe, that it was then diſcharged by ſpittle, and the pores of the ſkin, and mixed with oily particles of her fluids, which had acquired a quality analogous (if I may ſo ſpeak) to that of mercury, of ſtaining all linen. I am alſo apt to think, that this ſediment was the earthy matter, that gives the bones their ſolidity and hardneſs, which had been diſſolved by the ſame vitiated quality of the fluids, and evacuated by the emunctories already mentioned.

After great ſufferings, ſhe died the 9th of November. By the conſent of her huſband, I had her open'd, in the preſence of ſome of the moſt celebrated anatomiſts and academicians of this city. The operation was begun on the left tibia, cutting on the fore-part of it, from below the knee to its baſis. It was wonderfully alter'd; more or leſs ſoft in all its length; in ſome points intirely diſſolved, and its ſides not thicker than the griſtle of the ear. The ſpongy ſubſtance of its extremities ſupple, yielding to the leaſt preſſure. The reticular matter was quite deſtroyed. The peroné was intirely diſſolved in the middle, and only ſlight marks of its extremities remain'd. Inſtead of marrow, we found in all the bones a red thick matter, like coagulated blood

mixed

mixed with greafe. The rotula was intire, but very foft and fpongy; the condyles of the femur the fame. All the cartilages were found in their natural ftate. The head of the humerus was much diminifhed and flatten'd: Its middle part very fmall, pliable, foftened in all points, yet in fome friable. The cubitus and radius fuffer'd the fame alterations with the humerus. By ftretching all her limbs we laid them ftreight; but they foon after returned to their former curve. The phalanges of the fingers were not fo much foften'd, but were eafily cut, and bent like whale-bone. The femur was rather a flefhy body than a bone; its cavity was filled with a reddifh fuet, in-ftead of marrow, which, accumulated in different points, bulged out the flefhy fides. The capacity of the pelvis was much diminifhed; the bones, that compofe it, were foften'd, thicken'd, and contracted. The fpine kept its natural form; the vertebræ foft and fupple. The fternum, and all the cellular bones, feem'd folid, but could bend, and were eafily cut. The ribs, tho' foften'd, were ftill friable. Some of them, towards the fternum, were doubled over one another. The clavicles feem'd almoft cartilaginous. The fhoulder-blades were much thicker than natural, lefs broad, and intirely disfigured. The two protu-berances call'd acromion and coracoides almoft joined. The fcull-bones were eafily cut in flices, twice as thick as in their natural ftate. Both plates were joined in one, and no traces at all of a diploe. Their fubftance abounded with an extremely diluted ferum, eafily fqueezed out by a gentle preffure of one's fin-gers. The futures almoft obliterated: The bones of the bafis and face fhared in the calamity. The teeth

hard

hard as usual. The dura mater was incorporated with the bones. The brain not softer than ordinary: Its right hemisphere was by one third larger than the left; and hence, perhaps, the weakness of her left side, often manifested by pains, aches, defluxions, heaviness, falls on that side, and every illness, which she had from her infancy, beginning in some part thereof. When young, she fell upon her head down two pair of stairs. The membranes, that separate the two hemispheres of the brain, were much thicker than commonly.

In fine, all her bones were so soft, that the scalpel with very little force ran thro' the hardest of them; even the rocky apophyse of the ear bone, so called from its excessive hardness. Nothing extraordinary was found in the viscera; but their size diminished by the compression, and an universal cachexy.

There could be no cause assigned of this woman's disorder, as she gave no signs plain enough to prove either a scurvy, pox, or king's-evil, either hereditary, or acquired; her parents having lived healthy, the one to the age of eighty, and her mother being still alive, aged sixty, and in good health. She had three children, who died of disorders common to their age. One, 4 years old, died of the measles.

The case was looked upon as very extraordinary, and the patient was seen as a curiosity. It is certainly rare, but there have been some similar cases, which are cited in the *Abridgment of the Philosophical Transactions*, in the remark upon the like case presented to the Royal Society, by Mr. Silvanus Bevan,

Bevan, p. 458, &c. Vol. II. This differs from the other examples, by the ſediment of the urine, the ſtain on her linen, the preternatural ſituation of her limbs. Something very ſingular was, that ſhe did not blow her noſe perhaps once a month, even in her health; always ſlept with her mouth open'd, and her tongue hanging out. The manner in which ſuch diſſolutions of bones are accounted for, in the above-mention'd remark, ſeems the moſt rational, plain, and ſatisfactory, that can be given.

I attended this patient ſince laſt Auguſt with Dr. Morand, and kept a journal of our obſervations, hoping it may be of uſe to the public, and worth the notice of the curious.

Paris, Dec. 10, 1752.

Hoſty, *M. D.*

V. *An account of a Roman altar, with an inſcription upon it, found in April laſt at* York, *and communicated to the* Society of Antiquaries *by Mr.* Francis Drake, *F.R.S. As alſo a breif explication of the inſcription by* John Ward, *LL.D. Profeſſor of Rhetoric in* Greſham College, *and V. P. R. S.*

Read Feb. 1, 1753.

THIS altar was found, with other remains of antiquity, by ſome workmen, in opening a new and deep drain down the centre of a large ſtreet, called *Micklegate*, in the

city of *York*. Its hight with the pedeſtal, on which it ſtands, and which is made hollow to receive it, is fourteen inches and a quarter. But the breadth varies in ſeveral parts of it, according to their different form. On the top is an apex, with a volute on each ſide, and on the front a pediment over the inſcription. It is elegant for the workmanſhip, and well preſerved. And that a better judgement may be formed of it at this diſtance, Mr. *Drake* has ſent up a draught of it in its juſt proportion, with the inſcription upon it; as alſo another copy of the inſcription, taken off from the ſtone, by preſſing wet paper into the letters, and then delineating both them and the ſtops with a pencil. A copy of the draught, but reduced to one half of the ſize, accompanies this diſcourſe, to which I take leave to refer (1); and ſhall only ſubjoin here the inſcription itſelf in words at length, as I think it may be read.

Matribus Africis, Italicis, Germanicis, (2).
Marcus Minucius Ande.
miles legionis ſextae victricis,
gubernator legionis ſextae,
votum ſolvit libentiſſime merito.

1. By

(1) See TAB. I. *Fig.* 2.

(2) The abbreviation of this word being G N. as Mr. *Drake* aſſures me in a letter, which I received from him, ſince I wrote this diſcourſe; it may, I preſume, be read, as I have now given it. Tho, in the copy of the inſcription delineated upon the ſtone, the veſtiges of the letters ſeemed to be G A L. for which reaſon I then read it *Gallicis*.

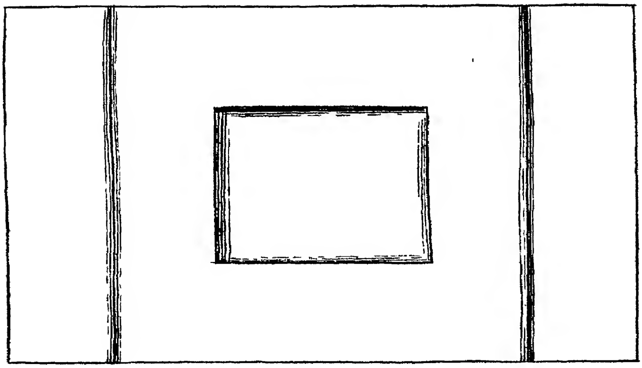
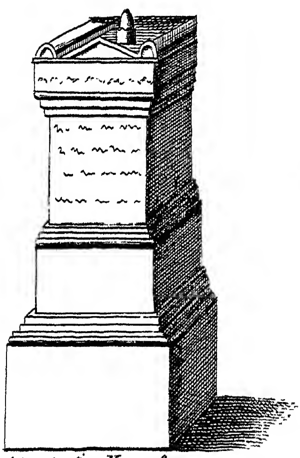


Fig 3 p 335.


LOCVMRELI
GIOSVMPERIN
SOLENTIAM·E
RVTVM
VIRTVTETN̄
AVGREPVRGA
TVMREDDIDIT
CSEVERIVS
EMERITVS Ↄ
PEG

Fig. 2. p. 124.

Fig. 1. p. 34.



A perspective View of Fig. 1.

J. Mynde sc.

1. By this inſcription, and many others of the like ſort, found in *Britain* and other countries, it appears, that theſe female deities, under the name of *Matres*, were worſhiped in moſt parts of the Roman territories. So that the obſervation of the learned *Spon* ſeems not to have been ſo well founded, when he ſais: *Notandum, has* Matres *fere tantum in Gallia et Germania fuiſſe cultas, raro enim alibi earum reperiuntur inſcriptae arae* (1). In moſt of theſe inſcriptions they are only called *Matres*, but in ſome *Deae Matres*; and frequently the country, or particular place, where they were worſhiped, is ſubjoined. Thus, in *Horſley*'s *Britannia Romana*, among the inſcriptions found in *Britain*, we have, *Matres domeſticae, Matres Alatervae, et Matres campeſtres*; as alſo *Matres tramarinae*, for *tranſmarinae* (2). Which laſt title, being in another inſcription writen *Matres tramai* (3), occaſioned the learned *Selden* to ſay: *Quid ſibi vellet, ne hariolari quidem auſus ſum* (4). And in the inſcription now under conſideration we meet with *Matres Africae, Italicae, Germanicae*.

Writers are not agreed in their opinion concerning theſe female deities, who were worſhiped under the character of *Matres*. *Spon* ſuppoſes they were deified women, who, while living, were thought to have the gift of prophecy. The reaſons for which opinion having been given at large in *Horſley*, I ſhall for brevity ſake take leave to refer to the paſſage (5).

E 2 But

(1) *Miſcel. erud. antiq. p.* 105.
(2) *Brit. Rom. Cumb.* XL. *Scotl.* XXIX. *Cumb.* LI.
(3) *Ibid. p.* 298.
(4) *De Diis Syris, Syntagm.* II. *c.* 2.
(5) *Pag.* 201. See alſo *Reineſ. Cl.* I. *n.* 175. *p.* 188.

But *Selden* applies them all to the *Dea Syria*, or *Mater Deorum*; whom St. *Auguſtin*, as he obſerves, takes for *Juno*, and ſais: *Tot eſſe Junones, quot ſunt ſimulacra* (1). Agreably to which notion we find ſeveral altars in *Gruter* inſcribed *Junonibus*, in the plural number (2). And *Plutarch* takes notice of the worſhip paid to the *Deûm Matri* at *Enguium* in *Sicily* (3); which *Cicero* ſeems to allude to, when he ſais, *Matris magnae fanum apud Enguinos eſt* (4). I ſhall only add further upon this head, that, as we meet with ſeveral inſcriptions, which have on them the title of *Matronae*, to whom they are addreſſed; *Spon* thinks, not improbably, that theſe *Matronae* were the ſame deities as the *Matres*, or *Matrae*, as they are ſometimes writen, who were indifferently worſhiped by each of thoſe titles, of which he has given a variety of inſtances (5).

2. The two firſt names of the perſon, who dedicated this altar, were doubtleſs *Marcus Minucius*; but the third, as here abbreviated, is uncertain. Tho I apprehend, that the two firſt letters in the ligature may be AN. which, with the two following DE. may ſtand for *Andegavus*, or, as it is ſometimes writen, *Andecavus*; as denoting the name of his country, now called *Anjou* in *France*.

3. This

(1) *Ubi ſupra.*
(2) *Pag.* XXIV. 1, 2, 3, 4, *et alibi.*
(3) *In vit. Marcelli.*
(4) *In Verr. lib.* IV. *c.* 44.
(5) *Ubi ſupra, pag.* 104.

3. This *Marcus Minucius* deſcribes himſelf by two characters or imployments; firſt, as *a ſoldier of the ſixth legion*, which was honored with the title of *victrix*; and then as *pilot of the ſame legion*, the epithet *victrix* not being repeated the ſecond time, as unneceſſary. It was not unuſual among the *Romans* for perſons to expreſs their ſeveral titles, or imployments of life, in the inſcriptions cut upon ſuch votive altars. Many of theſe are to be met with in collections of this kind; in ſome of which they are connected by the particle *et*; and in others not, as in the inſtance before us. I ſhall produce an example or two of each kind, to ſet this matter in a clearer light. Of the former ſort we have in *Gruter*:

I. O. M.
ET. FORTVNAE. SECVNDAE
CAES. M. ANTONINI. IMP
M. LICINIVS. COR. II. VIR
COL. DAC. SARMIZ. ET. FLA
MEN. DIALIS. SIGNVM. IO
VIS. EX. VETVSTATE. RE
STITVIT (1).

And again:

APOLLINI. AVG
SACR
FELIX. AVG. LIB. OPTIO
ET. EXACTOR. AVRI. ET
ARGENTI. ET. AERIS (2).

In

(1) *Pag.* VIII. I. (2) *Pag.* XXXVI. 8.

In both these instances the conjunction ET is expressed between the two characters, but in others it is omited. As in the same author:

HERCVLI INVICTO
M. CASSIVS HOR
TENSIVS
PAVLINVS PRAE. VRB.
XV. VIR
SACRIS FACIVND.
DONVM DEDIT (1)

So also in *Reinesius*:

IVNONI. REGIN
SACRVM
D. N. DIOCLETIANI IN
CLYTI. PRINCIP. SEM. AVG.
PINIVS PAVLLVS SILVANVS
VIR CLAR. PRAEF. VRB.
IVDEX. SACRARVM. COGNITIO
NVM. VOLVNT. EIVS CVR. (2).

And the like we meet with in *Horsley*, where one *Marcus Censorius Cornelianus* stiles himself, *Centurio legionis decimae Fretensis, praefectus cohortis primae Hispanorum* (3). In the first four of these instances the different characters given to the several persons are such, as might belong to them at the same time; but in the last they seem not compatible.

4. The

(1) *Pag.* XLVIII. *ult. in tab. priori.*
(2) *Class.* I. *num.* 282, *p.* 241.
(3) *Cumberl.* LXIII.

4. The title *gubernator*, or *pilot*, I do not remember to have met with in any other Roman inſcription. And notwithſtanding the firſt line is placed at ſome diſtance from the reſt; yet it may, I think, connect with them, without ſupplying the word *ſacrum*, in the following manner: *Matribus Africis*, *Italicis*, *Germanicis*, *Marcus Minucius*, etc. *votum ſolvit libentiſſime merito*. An inſtance of the like kind may be ſeen in *Horſley*, ending with the ſame letters, V S L L M, which he reads in the ſame manner (1).

It appears, by an inſcription in *Gruter* (2), and republiſhed by Dr. *Gale* (3), that this legion was tranſported from *Germany* to *Britain* in the reign of the emperor *Hadrian*, under the command of *Marcus Pontius*; who is there called, *Tribunus militum legionis ſextae victricis, cum qua ex Germania in Britanniam tranſiit*. The inſcription therefore upon this altar at *York* may, as I apprehend, refer to that voiage; and intimate to us, that *Marcus Minucius*, by whom it was erected, was then pilot to the legion. It is probable indeed, as *Horſley* obſerves (4), that

(1) *Scotland* XXVII. *Reineſius*, *Claſſ.* I. *num.* 3, reads the letters LL. *laetus libens*, and cites for it an inſcription in *Gruter*, pag. MLXXXIV. 4. which ends with the abbreviated words, LAET. LIB. FECIT. And there is another inſcription publiſhed by *Spon*, in his *Miſcell. erud. ant.* p. 107; and afterwards by *Fabretti*, *pag.* 690, *n.* III. which has the words LAETVS. LIBENS. at length. However *Reineſius* adds: *Vocabulum poſitivi geminatum infert ſuperlativum*; ſo BB. *bene bene*, is the ſame as *optime*; and LL. may ſtand for *lubentiſſime* or *libentiſſime*. And in like manner MM. is read in the ſuperlative *meritiſſimo*.

(2) *Pag.* CCCCLVII. 2.

(3) *Antonin. Iter Britann. pag.* 47.

(4) *Pag.* 79.

that upon its firſt arrival it made no ſtop in the ſouth, but marched directly by the uſual rout to *Hadrian*'s *vallum*; ſince there are ſeveral inſcriptions upon and near the wall, both in *Northumberland* and *Cumberland*, wherein this legion is mentioned. And in the following reign of *Antoninus Pius* we find, by ſeveral other inſcriptions, that it was in *Scotland*, and had a ſhare in building the wall there (1). Not long after it might very probably be ſtationed at *York*, where *Ptolemy* places it (2), who lived under the next emperor *Marcus Aurelius*, as we learn from *Suidas* (3). The legion therefore being thus ſettled, *Marcus Minucius* might then think it a proper time to pay his vows, formerly made to thoſe deities, whom he addreſſes in the inſcription cut upon this altar.

I thought it neceſſary to give this view of the matter, for the ſake of obviating an objection, which poſſibly may be made, as well with regard to the time of erecting the altar, as to the perſon, by whom it was erected. What I more eſpecially refer to, is the form of the letters G and L, as they appear upon the altar. For altho they are found in the like ſhape in ſeveral Britiſh inſcriptions publiſhed by *Horſley*, yet none of thoſe ſeem to be altogether ſo antient. There is one with the G, which that writer is uncertain, whether to refer to *Commodus* or *Caracalla* (4). And another with the L plainly relates to the reign of *Severus*, by the names of

(1) See *Horſley*, *Scotland*, IV, VII, XVIII.
(2) *Horſley*, *pag*. 359.
(3) *In voc.* Πτολεμαῖος, ὁ Κλαύδιος χρηματίσας.
(4) *Lancaſhire*, III.

of the consuls *Aper* and *Maximus*, who are mentioned in it, and answer to the year 207 of our common aera (1). The rest of them have nothing certain to determine their age; tho by some circumstances which attend them, they seem generally to agree better with later times. But if that particular form of either of those letters is not to be found in any of our *British* inscriptions hitherto published, before the reign of *Commodus* or *Severus*; it by no means follows, that it might not have been used sooner. And there was only the reign of *Marcus Aurelius*, that intervened between those of *Antoninus Pius* and *Commodus*, who in a few months was succeeded by *Severus*. Besides, as the sixth legion, after it settled at *York*, seems to have been statedly quartered there, till the *Romans* left *Britain* (2); I do not perceive, either to what other time, or person with the character of pilot to that legion, the erection of this altar can be ascribed. But this I leave to the further consideration of the curious, to judge of it as they please.

G. C. Nov. 9,
1752.

J. Ward.

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(1) *Cumberland*, XLIV.

(2) See *Horsley*, p. 79.

VI. *An Account of ſeveral Perſons ſeized with the* Goal-Fever, *working in* Newgate; *and of the Manner, in which the Infection was communicated to one intire Family; by* John Pringle, *M. D. F. R. S.*

Read Feb. 1, 1753

HAVING lately had an opportunity of ſeeing ſeveral caſes of the true goal-fever ariſing from the gaol itſelf, I thought it would not be improper to lay before the Society a ſhort account of the manner, in which thoſe perſons were ſeized, the chief ſymptoms and progreſs of the diſeaſe, with ſome remarks upon it, in order farther to illuſtrate what I have advanced elſewhere, concerning the danger ariſing from foul air; and the agreement of this diſtemper with what has been called the fever of the hoſpital, or more generally a malignant or peſtilential fever.

In the month of October 1750, a committee of the court of aldermen was appointed to inquire into the beſt means for procuring in Newgate ſuch a purity of air, as might prevent the riſe of thoſe infectious diſtempers, which not only had been deſtructive to the priſoners themſelves, but dangerous to others, who had any communication with them; and particularly to the courts of juſtice upon the trial of malefactors; whereof a fatal inſtance had occurred that year at the ſeſſions held in the Old-Bailey, when the lord mayor, two of the judges, and one of the aldermen upon the bench, with ſeveral other perſons then preſent, were ſeized with a malignant fever, and died.

The

The reverend Dr. Hales and I being consulted by the committee upon the point referred to them, and having visited the gaol in company with those gentlemen, it was then agreed, that, considering the smallness of the place, in proportion to the number of the prisoners, it would be proper to make a farther trial of the ventilator, and to have it worked by a machine, in the manner of a windmill, to be erected for that purpose upon the leads of Newgate.

This scheme was laid before the court of aldermen, and approved of, but not put in execution till near two years after. For on the 11 of July 1752, Dr. Hales acquainted Dr. Knight and me, that several of the tubes were finished, and that the machine had been going about six weeks; wherefore, being desirous to see the effects, he had appointed Mr. Stibbs the carpenter, employed in that work, to meet him that day at Newgate, and desired us to go along with. We went accordingly, and having visited several of the wards, we were all of us very sensible, that such as were provided with ventilating tubes were much less offensive than the rest that wanted them; and Dr. Hales and I could perceive a considerable improvement made upon the air of the whole gaol, since the time we had been first there with the committee. Some of the wards were so free from any smell peculiar to such places, that I am persuaded, were Dr. Hales's design completed, and a person appointed to regulate the sliders of the tubes, and to keep the machine in order, the usual bad consequences from foul and crouded goals, might in a great measure, if not wholly, be prevented in Newgate.

One of the wards allotted for the women had a ſmall room adjoining to it, in which they uſually ſlept. Both places ſeem'd at that time well air'd, tho' the latter was cloſe, and, if I miſtake not, without either window or chimney. The priſoners informed us, that, before this ward received the tube, this ſleeping-place had been very offenſive, but that ſoon after it became ſweet; and tho' upon the firſt working of the ventilator they had been more ſickly than before, they ſoon recover'd their health, and had preſerved it ever ſince. Now from this account we muſt not infer, that any danger will ariſe from a ſudden change of bad air for good; ſince this accident may be better accounted for from another circumſtance, which we were then likewiſe told of; *viz.* that this ward of the women had been ſupplied by a ventilating tube before thoſe in the lower ſtory, where the air being in a more corrupted ſtate, it had paſſed from thence thro' the ſeams of the floor, and other paſſages, to replace that, which was drawn off by the tube in the ward above: but that after the bad air was exhauſted, the benefit of the freſh air ſoon appeared, by the better health of the priſoners.

But as it was not my deſign in this paper to ſet forth all the advantages, that may be expected from the ventilator, I ſhall leave that ſubject to be treated of by the ingenious inventor of it; and ſhall only take notice, that the tubes from the ſeveral wards, uniting in one great trunk, convey all the putrid ſteams by that channel into the atmoſphere, through a vent made for that purpoſe in the leads of Newgate; and that tho' the wind was moderate during the time we ſtaid in the gaol, yet we obſerved, that the venti-

lator threw out a confiderable ftream of air, of a moft offenfive fmell.

Before we parted, Mr. Stibbs informed us, that Clayton Hand, one of his journeymen, whilft he was employed in fetting up the tubes, was feized with a fever, and carried to St. Thomas's hofpital, after lying fome days ill at his own houfe. Whereupon, apprehending that this man's ficknefs might be owing to the air of the gaol, Dr. Knight and I having the curiofity a few days after to go to St. Thomas's to make the inquiry, we found the patient fitting in one of the courts, recovered of his fever, tho' ftill weak, and had the following account from himfelf:

He faid, that upon firft finding himfelf indifpofed, he had left off work for fome days; but upon growing better he had returned to Newgate. That foon after happening to open one of the tubes of the old ventilator, which had ftood there for three or four years, fuch an offenfive fmell iffued from it, that being immediately feized with a naufea and ficknefs at his ftomach, he was obliged to go home, and that the night after he fell into a fever, in which he lay about eight days before his friends carried him to the hofpital. That becoming foon delirious, he recollected no other fymptom, fucceeding thefe mentioned, befides frequent retchings to vomit, a trembling of his hands, and a conftant head-ach. This man had taken no medicine before he came into St. Thomas's, and fince that time was attended by Dr. Reeves; but, as that gentleman was not then prefent, we were informed by the apothecary, that Clayton Hand had been admitted in the advanced ftate of a continued fever,

fever, attended with a ſtupor, and a ſunk pulſe, and that the fever had not left him till ſeveral days after his admiſſion. The nurſe's account was, that he had all along lain like one ſtupified, and that after the fever went off, he had continued for ſome time very dull of hearing. We could learn nothing certain about the preciſe duration of the fever, but from what the patient and his attendants told us, we collected, that he muſt have been ill between two and three weeks. So that from all theſe marks we made little doubt, but that this perſon had been ill of the true gaol-diſtemper; and were confirmed in our opinion by the following circumſtance.

In company with the convaleſcent was one Thomas Wilmot, another of Mr. Stibbs's journeymen, who had likewiſe worked in Newgate, and whom we remembered a few days before to have ſeen in that place, very active, and in perfect health. This man told us, he had come to ſee his companion, but as he apprehended himſelf in danger of falling into the ſame fever, he ſhould therefore be glad of our advice. Upon examination we found his tongue white, his pulſe quick, and that he complained of a pain and confuſion of his head, with a ſhaking of his hands, and a weakneſs in his limbs. He ſaid his diſorder had come on gradually, ſince the time we ſaw him in Newgate, but that he was then ſo very ill, he could work no longer. From which account it appeared to us, that this man had alſo caught the infection; but as the fever ſeemed not to be quite formed, we had hopes of ſtopping its progreſs: and with this view we adviſed him to take a vomit, and on the following night a ſudorific. He followed the

preſcription,

prefcription, and the effects fhall afterwards be mentioned.

After Wilmot had told us his own cafe, he informed us of the indifpofition of three more of his companions, who had been likewife employed by Mr. Stibbs in Newgate: whereupon we took their direction, vifited them, and found them all ill of the gaol-diftemper.

The firft was Michael Sewel, who lodged in the Swan-yard near Newgate. This man had been ten days confined to his bed, without taking any medicine. He was then delirious, and had the petechial eruption: but obferving, that he lay in a clofe, ill-air'd, and dirty room, without any attendants but his wife, then fuckling a child; we believed he had no chance to recover where he was, and therefore recommended his cafe to Mr. Stibbs, who procured his admiffion that day into St. Thomas's hofpital; where he alfo recover'd.

The fecond was Adam Chaddocks, who lay at a green-fhop in the little Old Bailey. He was taken ill on the fame day with the former, and had ufed no medicine. He had likewife the petechial fpots upon his breaft and back; and tho' he was not altogether infenfible, was affected with a ftupor attended with a funk pulfe, and other fymptoms of the diftemper. His landlady, who took care of him, informed us, that he had been troubled with retchings to vomit, and an head-ach from the beginning, and that for fome days paft he had been feized with a loofenefs, and that his ftools were very offenfive. As the room this perfon lay in was large and well-air'd, we did not think it neceffary to remove him, but

recommended

recommended him to the care of Dr. Pate, phyſician of St. Bartholomew's hoſpital, who attended him till he recover'd.

The third was John Dobie, apprentice to Mr. Stibbs, a lad about 15 years of age, who liv'd with his parents in a court by the White Bear in Canon-ſtreet. We ſaw him on the ſame day with the other two, which was the fourteenth of his ſickneſs, and the twelfth ſince he took to his bed. His mother told us, that ſome of the journeymen working in Newgate had forced him to go down into the great trunk of the ventilator, in order to bring up a wig, which one them had thrown into it; and that, as the machine was then working, he had almoſt been ſuffocated with the ſtench, before they could get him up. That upon coming home he complained of a violent head-ach, a great diſorder in his ſtomach, with retchings to vomit, which had never intirely left him. We found him extremely low, with a ſunk pulſe, a delirium, and an unuſual anxiety or oppreſſion about his breaſt. This laſt ſymptom we aſcribed to the opiates he was then taking for a looſeneſs, which had come on two or three days before we ſaw him. He being in no condition to be moved, and being beſides well attended by his mother, and in a well-aired chamber, we preſcribed to him there, and repeated our viſits, till he was quite free of the fever. It was obſervable, that before he was taken ill, he had been twice let down into the great trunk of the ventilator, when the machine was ſtanding ſtill, without complaining of any ill ſmell, or receiving any hurt thereby; but that the laſt time, when the machine was working, he immediately cried out he was ready to be

be ſuffocated; and the two men who helped him out, by receiving the foul ſteam from the trunk, were both ſet a vomiting ſo violently as to bring up blood.

On the 23 of Auguſt, Thomas Wilmot, above-mention'd, called upon Dr. Knight, and told him, that, after taking the vomit and ſudorific, he had immediately recover'd; but begg'd him to ſee his wife, who then lay ill of a fever, at his houſe in Snow's-fields, Southwark. The doctor ſuſpecting, that this woman's indiſpoſition might be owing to the contagion received from her husband, acquainted me with it, and carried me to ſee her. There we were informed, that Wilmot's daughter, a girl of eight years old, who lay with her parents, had been ſeized with a fever, ſoon after her father's recovery; that ſhe had been ill about a fortnight, and they believed had ſpots upon her breaſt, but that ſhe had recover'd without any medicine. That her mother had not only nurs'd her, but continued to lie with her; and that ſome time after the girl's recovery, the mother began to complain, and ſoon after fell into a fever; and that it was the twelfth day ſince ſhe was confined to her bed. This woman having the *petechiæ*, a ſtupor, with deafneſs, and a ſunk pulſe, there was no doubt of her being likewiſe infected with the diſtemper, and probably by her daughter. As ſhe had been without any aſſiſtance, we adviſed her husband to ſend for Mr. Breach, apothecary, in the Borough, who having ſerved in the hoſpital of the army during the war, was well acquainted with the nature of ſuch fevers; and having left directions with him, we did not return till after the criſis; which happen'd upon the 16 or 17 day from the time ſhe was confined to her bed.

Some time after this, Mr. Breach the apothecary informed us, that he was again employ'd in Thomas Wilmot's family; for that Elizabeth Marſhall, his ſiſter-in-law, after nurſing his wife, was taken ill of the ſame kind of fever, and deſired our aſſiſtance. This perſon we found in the ſame bed, and in the ſame condition, in which we had ſeen her ſiſter ſome time before; and in the room with her, in another bed, a ſon of Wilmot's, a boy of nine years old, ill of the ſame diſtemper. The former had been attack'd on the 15 of September, and the latter the day before. The woman's fever ran out the ordinary length of 16 or 17 days, but the boy's came ſome days ſooner to a criſis, and was all along of a milder nature. She recover'd very ſlowly, complaining of great weakneſs, deafneſs, and a confuſion in her head, the ordinary conſequence of theſe malignant fevers.

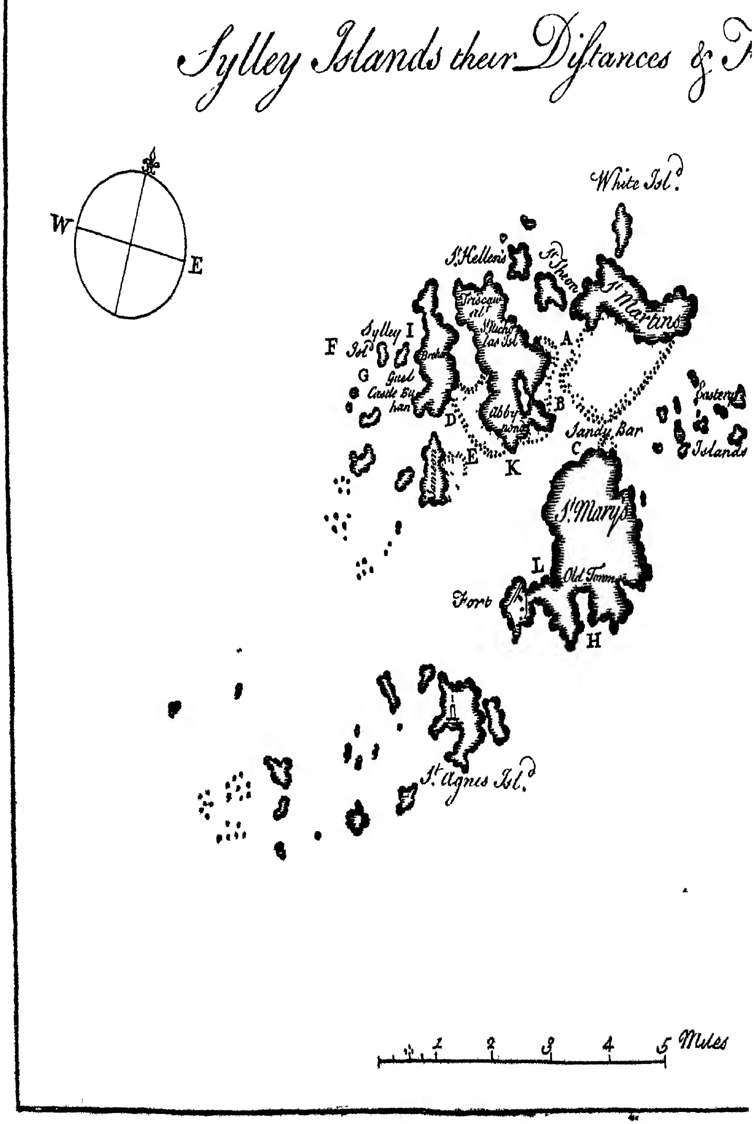
One day, in my return from this houſe, I call'd at St. Thomas's hoſpital, to inquire for one William Thomſon, a lad of about 16 years of age, who, as Wilmot then told me, was another of Mr. Stibbs's journeymen, and had been taken ill by working in Newgate, ſince the three he had mention'd before. This lad was recover'd, but not yet diſmiſs'd. He ſaid, that upon finding himſelf growing ill, he had left his work, and kept at home for about a week, complaining of a pain in the hinder part of his head, and in his back, of a trembling of his hands, and of reſtleſs nights; that his feveriſh indiſpoſition increaſing, he had been obliged to take to his bed, where he lay about eight days before he was ſent to the hoſpital. The apothecary added, that he had continued under their care about the ſame number of days before

the

the turn of his fever; that his pulſe had been extremely low all that time, and that they believed him to be in the utmoſt danger. He added, that the wife of Michael Sewel (the ſecond patient they had received of thoſe, who had been employ'd in Newgate) ſome days after her husband's admiſſion, came to ſeek advice for herſelf, and that her complaints had been the ſame with Wilmot's at the time we ſaw him: he added, that he had given her ſome medicines, but had heard nothing of her ſince.

On the laſt day of December Mr. Breach informed me, that about a month before, he had been call'd to attend Thomas Wilmot; but as he died before he ſaw him, he could give no other account of his ſickneſs, than as they told him; *viz.* that he had long been in a bad ſtate of health, and that at laſt he became feveriſh, and went off with a looſeneſs.

In the beginning of this month (January 1753) the widow applied to Dr. Hales and me, in order to have the diſtreſs of her family atteſted, and laid before the lord mayor, in hopes of having ſome proviſion made for them. Upon which occaſion we learned, that Thomas Wilmot her husband, after taking the ſudorific, ſo far recover'd as to work at his buſineſs; but that tho' he did not return to Newgate, yet his ſtrength would not permit him to continue at work above a day or two at a time; ſtill complaining of an head-ach, and pains acroſs his breaſt, or, as he expreſs'd it, about his heart, of a feebleneſs of his limbs, a ſhaking of his hands, and a conſtant drought. That notwithſtanding theſe ailments he went daily, till a week before he died, when he grew very weak, and more feveriſh, had ſometimes profuſe ſweats, and


Sylley Islands their Distances & T
W
E
White Isl.
St. Hellen's
St. Theon
St. Martins
Triscaw al
St. Nicholas Isl.
Sylley Isl.
I
F
G
Castle Bu han
A
B
D
Abby
Sandy Bar
C
Eastern Islands
E
K
St. Marys
L
Old Town
Fort
H
St. Agnes Isl.
1 2 3 4 5 Miles

Mens names.

Mr. Sewel	*	Mr. Morris
Mr. Hand	*	Mr. Bates
Mr. Willmot	* died,	Mr. Thompſon *
Mr. Letts		Burton, apprentice
Mr Chaddock	*	Dobie, ditto *.
Mr. Ruſt	*	

N. B. All thoſe mark'd *, had the gaol-diſtemper.

VII. *An Account of the great Alterations which the Iſlands of* Sylley *have undergone ſince the Time of the Antients, who mention them, as to their Number, Extent, and Poſition: In a Letter to the Rev.* Thomas Birch, *D.D. Secr. R. S. by the Rev. Mr.* Wm Borlaſe, *M. A. F. R. S.*

Reverend Sir,

Read Feb. 8, 1753.

HAVING made a little tour to the iſlands of Sylley laſt ſummer, and finding them very different from what I imagin'd, and from the deſcriptions given us of them, and the trade to them, by the ancients, had concluded them to be, you will excuſe my ſending you an obſervation or two, which occurr'd to me relating to their natural hiſtory, and the conſiderable alterations, which they muſt have undergone, ſince they were firſt taken notice of in hiſtory, and what I think may have been the cauſe of theſe alterations.

Theſe iſlands being ſo noted among the antients, I expected to find among the inhabitants a conſcious eſteem

eſteem of their own antiquity, and of their appearance in hiſtory before the other parts of Britain were at all known. I was not without hopes of finding old towns and caſtles, perhaps inſcriptions, and works of grandeur. But there is nothing of this kind; the inhabitants are all new-comers; not an old habitation worth notice; nor any remains of Phenician, Grecian, or Roman art, either in town, caſtle, port, temple, or ſepulchre.

All the antiquities here to be ſeen are of the rudeſt Druid times; and, if borrow'd in any meaſure, from thoſe eaſtern traders before-mention'd (ſuperſtition being very catching and infectious) were borrow'd from their moſt antient and ſimple rites.

We are not to think, however, but that Sylley was really inhabited, and as frequently reſorted to antiently, as the old hiſtorians relate. All the iſlands (ſeveral of which are now without cattle, or inhabitant) by the mains of hedges, walls, foundations of many contiguous houſes, and a great number of ſepulchral barrows, ſhew, that they have been fully cultivated, and inhabited.

That they were inhabited by Britons, is paſt all doubt, not only from their neighbourhood to England, but from the Druid monuments; ſeveral rude ſtone pillars; Circles of ſtones erect; Kiſt-vaens without number; Rock-baſons; Tolmêns; all monuments common in Cornwall and Wales, and equal evidences of the antiquity, religion, and original of the old inhabitants. They have alſo Britiſh names for their little iſlands, tenements, and creeks.

How came theſe antient inhabitants then (it may be aſk'd) to vaniſh, ſo as that the preſent have no

pretenſions

pretenſions to any affinity or connexion of any kind with them, either in blood, language, or cuſtoms? How came they to diſappear, and leave ſo few traces of trade, plenty, or arts, and no poſterity, that we can learn, behind them? This is what the curious will be ſolicitous to know; and two cauſes of this fact occurr'd to me whilſt I was at Sylley, which may perhaps ſatisfy their inquiries: The manifeſt incroachments of the ſea, and as manifeſt a ſubſidence of ſome parts of the land.

§ 2. The ſea is the inſatiable monſter, which devours theſe little iſlands, gorges itſelf with the earth, ſand, clay, and all the yielding parts, and leaves nothing, where it can reach, but the ſkeleton, the bared rock. The continual advances, which the ſea makes upon the low-lands, at preſent, are plain to all people of obſervation, and within theſe laſt thirty years have been very conſiderable. What we ſee happening every day may aſſure us of what has happen'd in former times; and from the banks of ſand and earth giving way to the ſea, and the breaches becoming ſtill more open, and irrecoverable, it appears, that repeated tempeſts have occaſion'd a gradual diſſolution of the ſolids for many ages, and as gradual progreſſive aſcendency of the fluids.

Again; the flats, which ſtretch from one iſland to another, are plain evidences of a former union ſubſiſting between many now diſtinct iſlands. The flats between Treſcaw, Brêhar, and Sampſon, (mark'd *D E* in the map) are quite dry at a ſpring-tide, and men eaſily paſs dry-ſhod from one iſland to another, over ſand-banks, (where, upon the ſhifting of the ſands, walls, and ruins are frequently diſcover'd) upon

H which

which at full sea there are ten and twelve feet of water. From the southern side of St. Martin's, (mark'd *A*) in the map annex'd, there stretches out a large shoal towards Trescaw and St. Mary's; and from St. Mary's, a flat call'd Sandy-bar (mark'd *C*) shoots away to meet it; and between these two shoals there are but four feet of water, in the chanel call'd Crow-sound: All strong arguments, that these islands were once one continued tract of land, tho' now, as to their low-lands, over-run with the sea and sand. History confirms their former union. "The isles Cassiterides (says "Strabo *) are ten in number, close to one another; "one of them is desert and unpeopled, the rest are "inhabited." But see how the sea has multiplied these islands; there are now reckon'd one hundred and forty: Into so many fragments are they divided, and yet there are but six inhabited.

§ 3. But no circumstance can shew the great alterations, which have happen'd in the number and extent of these islands more than this; *viz.* that the isle of Sylley, from which the little cluster of these Cyclades takes its name, is no more at present than a high rock, of about a furlong over, whose cliffs hardly any thing but birds can mount, and whose barrenness could never suffer any thing but sea-birds to inhabit it. How then came all these islands to have their general name from such a small and useless plot? From the disposition of the rocks and islets here, and allowing the alterations before suggested, we may answer this question, which would otherwise be extremely difficult to solve.

From

* Geogr. lib. 5.

From the ſouthermoſt hill of Brêhar I obſerved the Guêl hill, and the iſle of Guêl (mark'd *I*) ſtretching away towards the little iſle of Sylley, and with it making a curve, of which Sylley is the head-land: from the furthermoſt hill of Brêhar, a promontory ſhoots out in the ſame ſweep, and at the extremity, a vaſt rocky Tor call'd the Brêhar, behind which a ledge of many pointed rocks ſhew themſelves above water, intimating the former connexion, and with what great devaſtations the furious ocean has diſſolv'd it. So that Sylley, which is now a bare rock, and ſeparated from the lands of Guêl and Brêhar by a narrow frith of ſea, was formerly join'd to them by low necks of lands, being the rocky promontory of one large iſland now broke into ſeven.

To purſue this conjecture (if I may call that ſo, which has ſo many reaſons to ſupport it) a little farther: When all theſe ſeven iſlands, Sampſon, Brêhar, Treſcaw, St. Elen's, Theon, St. Martin's, and St. Mary's, made but one, that one went by the name of Sylley, or ſome name of like ſound and derivation *; and, having ſome little iſlands ſcatter'd round it, imparted its name to them; whence, what were called by the Greeks Caſſiterides, were nam'd by the Latin authors Sygdeles, Sillinæ, or Silures, from the Britiſh name, as I apprehend, which they found them call'd by

* Sûl-lêh ſignifies in Cornu-britiſh, flat rocks, of, or dedicated to, the ſun. So the mount of St. Michael was call'd in Cornu-britiſh Din-ſûl, the hill conſecrated to the ſun; and, as I conjecture, the vaſt flat rocks, common in theſe iſlands, had the like dedication: For, upon the ſurface of one of them, there are the remains of an antient Druid temple. It is one plane of rock, edg'd with nine vaſt ſtones (the reſt taken away) planted in a circular line, meaſuring 172 feet from north to ſouth, and 138 feet from eaſt to weſt.

among the natives. I muſt go farther ſtill, and obſerve that the promontory (at preſent call'd Sylley iſland) lying weſternmoſt of all the high lands, was the firſt land of all the iſlands diſcern'd by traders from the Mediterranean, and Spaniſh coaſts, and, as ſoon as diſcover'd, was ſaid to be Sylley; nothing being more uſual with ſailors, upon their firſt ſeeing land, than to call the part by the name of the whole; of which I will not detain you with proofs. But, when this conſiderable iſland called Sylley was broke to pieces, the great portions became inhabited, and requiring diſtinct appellations, had firſt Britiſh names, as Brêhar, Treſcaw, Enmor; but as ſoon as the regulars were placed here (probably in Athelſtan's reign, or immediately after) were call'd, according to the religion of the times, after the names of particular ſaints. The chief diviſion was intituled St. Mary's, in honour of the holy virgin-mother; the others dedicated to St. Nicholas, St. Martin, St. Theon, and ſo on; but this remarkable promontory (now Sylley iſle) being in no wiſe fit for habitation or devotion, was dedicated to no ſaint, but left to enjoy its antient name; and, notwithſtanding the modern Chriſtian dedications, ſailors went on in their old way; this high land is ſtill call'd Sylley, and the iſlands in general are ſtill denominated, from what was antiently their principal, Sylley iſles.

It muſt have been a very diſpiriting circumſtance to the old inhabitants, to ſee the ocean ſo continually eating away their low-lands, in which they had their treaſures of tin, their houſes, and ports: but this gradual decay was not the only misfortune, which attended

them,

them, neither will it account for the following phænomena.

§ 4. It has been mentioned before, that upon ſhifting of the ſands in the chanel, walls and ruins are frequently ſeen: There are ſeveral phænomena of the ſame nature, and owing to the ſame cauſe, to be ſeen on theſe ſhores, as particularly a ſtrait-lin'd ridge like a cauſeway, running croſs the old town creek (mark'd *H*) in St. Mary's, which is now never ſeen above water; ſo that the ſubſidence, if any, has been different in different places. From the iſland of Sampſon I ſaw the foundations of hedges (ſo we call the ſtone-fences of our fields, which are not built of maſonry and cement) running on in a ſtrait line croſs the frith *E*, towards Treſcaw iſle, till they were hid in ſand; which ſand, when in its full tide, has from ten to twelve feet water on it. Now we cannot ſuppoſe, that the foundation of theſe hedges was laid as low as high-water mark (for who would build fences upon ſo dangerous a level?) and if, at a medium, we ſuppoſe them to have been laid only ſix feet above the full tide, I am perſuaded it will not be thought an unreaſonable calculation. Here then we have the foundations, which were ſix feet above high-water mark, now ten feet under, which together make a difference as to the level of ſixteen feet. To account for this, the ſlow advances and depredations of the ſea will by no means ſuffice; we muſt either allow, that the lands incloſed by theſe hedges have ſunk ſo much lower than they were before; or elſe we muſt allow, that, ſince theſe lands were incloſed, the whole ocean has been raiſed ſixteen feet perpendicular;

which

which laſt will appear, Sir, I believe, to the judicious, much the harder and leſs tenable ſuppoſition of the two.

Here then was a great ſubſidence; the land betwixt Sampſon and Treſcaw ſunk at leaſt ſixteen feet, at a moderate computation. This ſubſidence muſt have been follow'd by a ſudden inundation; and this inundation is likely not only to have deſtroy'd a great part of the inhabitants, but to have terrified others who ſurvived, and had wherewithal to ſupport themſelves elſewhere, into a total deſertion of their ſhatter'd iſlands. By this means, as I imagine, that conſiderable people, who were the aborigines, and carried on the tin-trade with the Phenicians, Greeks, and Romans, were reduced to the laſt gaſp. The few poor remains of this deſolation, by their neceſſary attention to food and raiment, muſt ſoon have loſt ſight of their antient proſperity; (for trade will produce plenty, and plenty rightly uſed will make people happy) and the faint remembrance, that was left of what the iſlands had been before, expired of itſelf, in an age or two, through the indigence of the inhabitants.

§ 5. That ſuch an inundation has happen'd here, is ſtill more plain, becauſe theſe iſlands are no longer, what they were antiently, fertile in tin; nor are there any remains of ſuch and ſo many antient workings as could maintain a trade, ſo greedily coveted by ſome of the antients, and ſo induſtriouſly concealed by others.

There are no mines to be ſeen in any of theſe iſlands, but only on one load (ſo we call our tin veins) in Treſcaw iſland, and the workings here are very

very inconſiderable, and not antient; and indeed the ſides of the cliffs, and the ſtrata of bare rocks, do not ſhew, that ever there have been any conſiderable workings there for tin, or give us the leaſt promiſing circumſtance to encourage tin-adventurers. For among ſuch numbers of rocks and cliffs, as I paſs'd over in St. Mary's, and the off-iſlands, nothing ſurpriz'd me more, than that there ſhould be ſo few veins in ſtrata formerly ſo famous for tin. In the cliffs of Cornwall 'tis very different; you cannot walk on any beach, without perceiving veins of one ſort or other, in the clay, rubble, or rock; but here in Sylley it is generally one continued rock, and the interſtices ſo cloſe, that ſcarce a knife can get between.

Beſides the load above-mention'd in Treſcaw, I ſaw a very narrow one in the ſame iſland under a place call'd Oliver's battery (marked *K*) but could perceive no metal, either by inſpection, or by more than ordinary weight. I ſaw two veins, about two inches wide, running thro' the rocks on the back of St. Mary's pier; and a gentleman in company thought he had found one in Porthmellyn (*L*); and theſe are all we could diſcover, tho' our attention this way ſeldom left us.

There is one place on Dolphin-Downs, in Treſcaw, where they work'd for tin in the antient manner, which was by laying open the ground in the ſame way as we now work ſtone quarries; and this is every thing I could perceive, in all theſe iſlands, which look'd like a working for tin. It muſt therefore be matter of wonder where the Phenicians, Greeks, and Romans, could have found ſuch a plenty of that uſeful

metal.

metal. Whatever resources they had from Cornwall, (formerly reckoned, as I have great reason to think, among the Cassiterides) great part of their tin must doubtless have come from these islands; but where it was found is uncertain. Nothing now appears above-ground, which can satisfy such an inquiry. The story of the Phenician vessel mention'd by Strabo to have purposely run ashore, and risqu'd the men as well as lost the ship, rather than discover to the Romans the trade to these isles, is well known, and proves beyond all doubt the commerce to have been very advantageous. That the natives had mines, and work'd them, appears from Diodorus Siculus, *lib.* 5. *ch.* 2. and from Strabo (*Geog. lib.* 3.) who informs us, that Publius Crassus sailing thither, and observing how they work'd their mines, which were not very deep, and that the people lov'd peace, and at their leisure navigation, instructed them how to carry on this trade to better advantage: That is (if I understand him rightly) seeing their mines but shallow, yet well worth working deeper, taught them how to pursue the metal to a greater depth. The question then is, what is become of these mines? And how shall this question be answer'd, but by confessing, that the land, in which these mines were, is now sunk, and bury'd under the sea?

I am not fond of introducing earthquakes, or calling in the powerful subterraneous world to account for the superficial; but where there has been evidently a great subsidence of the earth's surface, I am very willing to refer to better judges than myself, whether it can be accounted for at all, without a previous trembling and concussion of the earth.

Proofs

Proofs are much better drawn from nature than tradition; but yet I cannot help obferving, that what nature declares in this cafe, tradition feems to confirm; there being a ftrong perfuafion in the weftern parts of Cornwall, that formerly there exifted a large country betwixt the Land's-end and Sylley, now laid many fathoms under water. The particular arguments, by which they would fupport this tradition, I forbear to trouble you with, as they may be feen in Mr. Carew's *Survey of Cornwall*, p. 3. and in the laft edition of Camden, p. 11. But, if I were to produce them, it fhould only be to prove, that there was fuch a tradition, and not as proofs of the matter of fact; for of that I am very dubious; the Caffiterides, by the moft antient accounts we have of them, appearing always to have been iflands. I know it is the opinion of fome naturalifts, that all iflands were formerly join'd to the continent; but we may, at leaft in my opinion, as well fuppofe the feveral parts of the continent, which are now feparated by the branches of the fea, to have been once conjoin'd; which will prove a great deal too much; the prefent intermixtures of land and water being a much more gracious and focial difpofition, and for that reafon more likely to be the true and original one, than fo clofe and compact an union; and therefore, where there are not fhoals, and other evidences of a connexion (as there are none betwixt the Land's-end and Sylley) I think it the fafeft way not to fuppofe any.

But tho' there are no evidences, to be depended upon, of any antient connexion of the Land's end and Sylley, yet that the caufe of that inundation, which deftroy'd much of thefe iflands, might reach alfo to the Cor-

niſh ſhores, is extremely probable; there being ſeveral evidences of a like ſubſidence of the land in Mount's-bay. The principal anchoring-place, call'd a lake, is now a haven, or open harbour. The mount, from its Corniſh name *, we muſt conclude to have ſtood formerly in a wood, but now at full tide is half a mile in the ſea, and not a tree near it; and in the ſandy-beach betwixt the Mount and Penzance, when the ſands have been diſperſed by violent high tides, I have ſeen the trunks of ſeveral large trees in their natural poſition, the ſurface of their ſection worn ſmooth by the agitation of the water, ſand, and gravel, as if cut with an axe, upon which, at every full tide, there muſt be twelve feet water; ſo that the ſhores in Sylley, and the neighbouring ſhores of Cornwall, are equal and concurrent evidences of ſuch a ſubſidence, and the memory of the inundations, which were the neceſſary conſequences of it, is preſerv'd in tradition; tho' like other traditions, in proportion to their age, obſcur'd by fable.

I ſhould now make an apology, Sir, for troubling you with this ſketch of the alterations, which the Sylley iſles have ſuffer'd ſince the age of the antient hiſtorians: but becauſe I have no other view in it than teſtifying my reſpect to that honourable Society, in which you deſervedly hold ſo eminent a ſtation, that muſt be my apology. I only beg leave to obſerve, that altho' theſe iſlands are neither of that extent they were formerly, nor fruitful of tin, nor ſtor'd

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* Guavas lake, ſignifying the grey rock in a wood.

ſtor'd with ſuch antiquities, as one might expect from their long commerce with the Phenicians, Greeks, and Romans; yet I found them well worth ſeeing: they are inhabited by an induſtrious, apprehenſive people; their lands, fortifications, and commerce, are capable of great and eaſy improvements, and their harbours are of the utmoſt importance to the navigation of this kingdom. I am,

SIR,

Ludgvan, Dec. 21, 1752.

Your moſt obedient ſervant,

Wm. Borlaſe.

Extract of another Letter of Mr. Borlaſe *to the Rev. Dr.* Lyttelton, *Dean of* Exeter, *and F. R. S.*

Read Feb. 8, 1753.

THAT there has been ſuch a ſubſidence of the lands belonging to theſe iſlands, (as is before ſuppoſed) the preſent ruins of the iſlands teſtify. That this ſubſidence reach'd even to the Mount's-bay, and laid under water a great part of the low lands, then woody, there being now 10 feet water, (as at Sylley over the hedges, ſo here over the trunks and roots of trees) I have ſuch convincing reaſons to lay before you at a proper time, as will exclude all doubt. So that the ſhores in Sylley, and the ſhores in Cornwall, are equal proofs of ſuch inundation, and the memory of it is preſerv'd in tradition, tho', like other traditions, obſcur'd in fable.

When this inundation happen'd, it is perhaps in vain to inquire; but two pieces of hiſtory occur to me, which poſſibly may lead us near the time. In the time of Strabo and Diodorus Siculus, their commerce ſeems to have been in full vigour; "Abun-"dance of tin carried in carts," ſays Diodorus Siculus: — "But ten iſlands in all, (ſays Strabo) and "nine of thoſe inhabited." The deſtruction therefore of Sylley muſt be plac'd after the time of theſe authors; that is, after the Auguſtan age: but at what time after, I find nothing as yet can determine.

Plutarch indeed (of the *Ceſſation of Oracles*) hints, that the iſlands round Britain were generally unpeopled in his time. If he includes Sylley among them, then this deſolation muſt have happened between the reign of Trajan, and that of Auguſtus. However that be, Sylley, tho' not intirely deſtitute of inhabitants, ſeems to have been noted for two or three baniſhments during the empire of the Romans in Britain.

It continued neglected, till trade began to thrive, ſhipping increaſe, and naval wars to be carried on in this weſtern world. Then its commodious ſituation at the opening into both the chanels ſoon ſhew'd of what importance it was for Britain to poſſeſs it, and how dangerous it might be to the ſafety and trade of Britain, if in an enemy's hand.

This ſeems to be the reaſon, why Athelſtan made a voyage to, and conquer'd, theſe iſlands. That prince was taught by his grandfather Alfred's wiſe maxims, that the proper and natural ſecurity of Britain lay in the royal navy, and its riches in traffick; and he ſaw, that neither of them could be well pro-

vided

vided for, if Sylley were not ſubdued as well as Cornwall.

VIII. *An Account of Mr.* Appleby's *Proceſs to make Sea-Water freſh; with ſome Experiments therewith; communicated to the* Royal Society, *by* W. Watſon, *F. R. S.*

Read Feb. 8, 1753.

THE making ſea-water freſh has been frequently attempted, and ſeveral accounts thereof, from time to time, been communicated to the Royal Society. I thought it therefore not improper to lay before you Mr. Appleby's proceſs for this purpoſe, in order to its being preſerved in the journal-books of the Society. To this I have ſubjoined ſome experiments upon the water prepared in Mr. Appleby's manner, made by Mr. Michael Clark, operator at Apothecaries Hall, a perſon extremely well verſed in the theory and practice of chemiſtry, at the deſire, and under the inſpection, of the cenſors of the college of phyſicians, to whom Mr. Appleby's proceſs was referred by the lords commiſſioners of the admiralty.

Mr. Appleby's proceſs.

INTO twenty gallons of ſea-water put ſix ounces of a fixed alcali, prepared with quick-lime as ſtrong as *lapis infernalis*, and ſix ounces of bones calcined

to

to whitenefs, and finely powder'd. With a flow fire, draw off in a common ftill fifteen gallons.

Mr. Appleby conceives, that the alkali here employed is the beft adapted to prevent the bituminous matter in fea-water from rifing by the heat in diftillation.

Mr. Clark's experiments.

INTO a fpoonful of the diftilled fea-water he put twenty drops of a folution of filver in aq. fortis: He likewife did the fame with the like quantity of common water diftilled. There appeared no change in either, and both retained their tranfparency.

This demonftrates, that the diftilled fea-water is by the procefs intirely freed from marine falt, or its acid fpirit. For, if we take a fpoonful of common diftilled water, and add the leaft particle of fea-falt, with the point of a penknife, and then drop into the mixture one or two drops of the folution of filver, it will appear turbid and milky.

From the number of animal bodies conftantly perifhing in the fea, it may reafonably be fufpected, that a volatile urinous fpirit may be retained in this diftilled water; and this is evident from the following experiment:

Into a fpoonful of diftilled fea-water drop ten drops of a ftrong folution of fugar of lead, and the mixture immediately becomes turbid and milky.

Into another fpoonful of common diftilled water, with two drops of fpirit of fal ammoniac, add ten drops of a folution of fugar of lead: and this mixture had the fame appearance with the foregoing.

If into a ſpoonful of common diſtilled water is dropp'd one drop of oil of tartar *per deliquium*, and then added ten drops of a ſtrong ſolution of corroſive ſublimate, the mixture will immediately become turbid and brown, and with a few drops of the ſolution of ſilver, it will be precipitated, and turn milky. It is a volatile alkali therefore, and not a fixed one, that is contained in this water.

The ſolution of ſilver will not diſcover a volatile alkali contained in water, but very plainly a fixed one.

A ſolution of ſugar of lead will not diſcover a ſmall quantity of marine ſalt or ſpirit, till we add more.

A ſolution of ſublimate will manifeſt both a volatile and fixed alkali.

IX. *Extract of a Letter from Signor* Camillo Paderni, *to Dr.* Mead, *concerning the Antiquities dug up from the antient* Herculaneum, *dated from* Naples, Nov. 18, 1752. *Tranſlated from the* Italian.

Read Feb. 8, 1753.

THE things, of which I have the charge, are many, and extraordinary; conſiſting of

Metals; that is, bronzes, ſilver and gold of all kinds, of excellent workmanſhip.

Beautiful cameo's and intaglio's.

Glaſs of all ſorts.

Various

Various productions of the earth; ſuch as, grain, beans, figs, dates, nuts, piſtachio's, almonds, rice, bread.

Colours for painting.

Medicines in pills, and other forms, with their marks.

A phial of oil.

Gold lace, perfectly well preſerved, and extremely curious, on account of its being made with maſſy gold, ſpun out, without any ſilk, or other yarn.

Soap, bran, and a variety of other things, which it were tedious here to enumerate; but there will be a relation of the whole publiſhed, which I ſhall immediately ſend to you; as I hope you have received the book of Monſeigneur Bajardi, already ſent, altho' of little ſignificance *.

It is not a month ago, that there have been found many volumes of *papirus*, but turn'd to a ſort of charcoal, ſo brittle, that, being touched, it falls readily into aſhes. Nevertheleſs, by his majeſty's orders, I have made many trials to open them, but all to no purpoſe; excepting ſome *words* §, which I have picked out intire, where there are divers *bits*, by which it appears in what manner the whole was written. The form of the characters, made with a very black tincture, that overcomes the darkneſs of the charcoal, I ſhall here, to oblige you, imitate in

* The words in the original letter are, "Come ſpero che avera "ricevuto il libro de Monſ. Bajardi inviatogli a benche non ſerva."

§ I ſuſpect an inaccuracy here in the original, and that he meant, "excepting ſome bits, which I have pick'd out intire, "where there are ſeveral words, &c."

in two ſhort lines; my fidelity to the king not permitting me to ſend you any more.

N·ALTERIVS·DVLC
DEM·CVRIS·CRVDE

This is the ſize and ſhape of the characters. In this bit there are eight lines. There are other bits with many other words; which are all preſerved in order for their publication.

There have been found likewiſe very lately three beautiful ſtatues of marble, and one of them excellent: Six heads of bronze, of which there is one, that gives hopes of finding the ſtatue it belongs to. It is a young Hercules, of a kind of work, that has no fellow in the way of metal, having the hair finiſhed in a ſurpriſing manner. Likewiſe ſeveral little figures of metal; a ſiſtrum very neat and well preſerved; and there is not a day paſſes, in which they do not bring to me ſome curioſities newly found.

X. *A Tranſlation and Explanation of ſome Articles of the Book intitled,* Theorie de la Figure de la Terre; *by Monſ.* Clairaut, *of the* Royal Academy *of* Sciences *at* Paris, *and F. R. S.*

Read Feb. 15, 1753.

MR. Short, in his account of Father Friſius's *Diſquiſitio mathematica in cauſam phyſicam figuræ et magnitudinis telluris noſtræ,* having reported that philoſopher's ſentiments

on my reflections upon the same matter, without taking the trouble to examine whether they were founded upon the truth or not, I find myself under the necessity of laying before the Royal Society the passages of my book, which, having been misunderstood by F. Frisius, have occasioned the misconstruction, which he has made of my sentiments, either upon the trust I give to the actual operation made for discovering the figure of the earth, or Sir Isaac Newton's theoretical inquiries about the same subject.

The expressions of Father Frisius, referr'd to by Mr. Short, are as follow:

"Quia tamen plerique omnes hucusque, aut nihil "pro figura telluris determinanda ex iis observationi-"bus deduci posse cum geometra celeberrimo Rug-"gero Boscovik autumârunt, aut exinde cum Ill. "Clairaut, Bouguer, aliisque, contra incomparabilem "virum ac prope divinum Isaacum Newton insur-"gentes, admirabilem ipsius theoriam facto minus "respondentem dixerunt, assignatamque in prop. 19. "lib. 3. *Princip. Mathem.* terrestrium axium pro-"portionem à vera absonam omnino esse, alios mihi "observationibus parum, alios nimis tribuere visum "est, omnes ferme oppositis erroribus peccâsse, ubi "res neque aurificis lance, neque molitoris, ut aiunt, "statera librandæ sunt."

This, when compared with the propositions of my theory, which they are relative to, will appear, I hope, quite incoherent: and I cannot shew it more clearly, than by translating the last chapter of my book, to which F. Frisius refers the reader.

For

For the better underſtanding of that chapter, it is proper to know, that the chief reſults of the precedent inquiries are theſe theorems:

1. Suppoſing the earth in its former ſtate compoſed of ſeveral fluids of different denſities, and ſettled all in equilibrium by the laws of gravity and centrifugal forces, the ſurfaces ſeparating the different mediums will always affect the form of a curve; which is ſo near to the ellipſis, that it may be ſuppoſed ſo, without any error of the leaſt moment.

2. That, in the caſe of the denſer fluids being nearer to the center, as hydroſtatics require, the ſpheroid will always be leſs flat than in the homogeneous one, and *vice verſa.*

3. And as to the diminution of the gravity from the pole to the equator, it will always follow the oppoſite rule; *viz.* if the ſpheroid be denſer towards the center, the gravity will decreaſe in a leſs ratio than in the homogeneous ſpheroid, and *vice verſa.*

4. That if δ repreſent the fraction found out for the difference of diameters, $\frac{1}{115} - \delta$ will expreſs the total diminution of the gravity from the pole to the equator, not only in the caſe of the ſpheroid ſuppoſed originally fluid, but in any ſuppoſition of variation for the denſities and proportion of the diameters of the beds, provided they be elliptical.

Theſe premiſed, let us proceed to the ſaid laſt chapter of the theory of the earth's figure; in which the principles laid down in the preceding chapters are compared with the obſervations.

§ LXVIII. *For the diminution of the gravity from the North to the South.*

It has been ſeen in the preceding chapter, that when a ſpheroid is not ſuppoſed homogeneous, the diminution of the gravity from the pole to the equator will be greater than in the caſe of homogeneity. Hence, if my theory holds in our globe, the whole decreaſing of the gravity will be equal to $\frac{1}{230}$ or greater, and never leſs; ſince the ratio of 230 to 231 will (§ XXI.) expreſs the ratio of the action of gravity at the equator and pole, when the ſpheroid is homogeneous.

And this concluſion of my theory quite agrees with experience; for, from all the obſervations relating to the gravity made in ſeveral places of the globe, either by actual meaſures of the ſecond pendulum, or by the difference of duration of the ſame pendulum's vibrations, it appears, that the gravity decreaſes from the north to the ſouth in a greater ratio, than it would be if the total diminution from the pole to the equator were only $\frac{1}{231}$.

§ 69. *For the proportion of the two diameters.*

Suppoſing, as in the precedent chapter, the earth originally fluid, it follows, from the § LXV. that the ratio of the two diameters cannot exceed that of 230 to 231; ſince, § XX. 230 to 231 is the ratio in the caſe of the homogeneous ſpheroid; and as the menſurations of the gravity cannot agree with the ſuppoſition of the homogeneity, the diameters of the earth ought to be in a ratio leſs than 230 to 231.

Without

" Without adhering to the ſuppoſition of the earth's being formally fluid, but admitting, as in the chap. 3 and 4, all generality poſſible in the variation of denſity and ratio of diameters of the beds or ſtrata laid down from the center to the ſurface, there will ſtill happen a difference of the diameters leſs than $\frac{1}{230}$. For, by § L. the total diminution of the gravity from the pole to the equator being ſubtracted from $\frac{1}{115}$, the remainder is the difference between the diameters. Now the diminution of the gravity having been found greater than $\frac{1}{230}$, the ellipticity or difference of diameters ought to be leſs than that fraction, and conſequently the ratio of diameters leſs than 230 to 231.

That conſequence of my theory is not ſo happy as the preceding; for the degree meaſured in the north compared to that of France give the two diameters as 177 to 178, which ratio is greater than 230 to 231, inſtead of being leſs, as the theory would require.

As the meaſures made in the north have been performed with great care and exactneſs, their reſult ſeems at firſt to be preferred to that of my theory. But a reflection upon the errors unavoidable in actual meaſures, and an examination of the limits of theſe errors, will ſhew, that, without violating the meaſures, they would be brought nearer the theory, and even agree with it.

For, by a convenient calculation, it will be found, that a diminution leſs than 60 toiſes, made to the difference between the degrees of Paris and Tornea, would give the diameters in the ratio of 230 to 231. And if it be conſidered, what is the ſmallneſs of an error of 60 toiſes, when divided in two operations, which require

require ſo great a number of aſtronomical and geographical obſervations, it will be thought, that an error a little larger may be ſuppoſed, without diſparaging either our operation, or Mr. Picard's; and thus theory and experience would agree.

Suppoſing, for example, that the difference between the degrees of Paris and Tornea has been found too great by 80 toiſes, the difference between the two diameters will come out of about $\frac{1}{260}$, which, ſubtracted from $\frac{1}{115}$ gives $\frac{1}{206}$ for the diminution of the gravity from the pole to the equator. And ſuch a concluſion would agree pretty well with the obſervations made in France and Lapland with the excellent clock of Mr. Graham.

However, altho' the errors to be ſuppoſed in the operations, to reconcile them with my theory, be in themſelves ſmall enough, I ſhall abſtain from aſſerting, that they have been committed. It is a fact not to be decided, till after the reſult of the obſervations, which are expected from Peru. For the great difference, which is to be found between the degrees of Quito and Tornea, is the only means of knowing, whether the diameters be leſs or greater than 230 to 231.

Were the queſtion only to demonſtrate the flatneſs of the earth, the meaſures of the degree of Paris and Tornea would be full ſufficient; but to diſcover the true ratio of diameters, is what can be performed only by the compariſon between the degrees, whoſe mutual diſtance is the greateſt.

Such a ratio once fixed, if it happen to be leſs than 230 to 231, it will be very eaſy, by the preceding theory, to imagine ſome hypotheſis for the inſide of the

the earth, which ſhall agree with both theory and obſervation, whether admitting the ſuppoſition of the original fluidity of the globe, or not.

But if the diameters were found undoubtedly in a greater ratio to one another than 230 to 231, I own, that not only the theory eſtabliſhed in this ſecond part of my book muſt be abandoned, but it would be very difficult to reconcile the meaſures of the pendulums with thoſe of the degrees in Sir Iſaac's ſyſtem. And I dare ſay, that the ſucceſs in that caſe would hardly depend upon any natural hypotheſis.

The ſubſequent LXX article containing only a proof, that the preceding theory agrees with any ratio between $\frac{1}{23}$ and $\frac{1}{9}$, for the quantity, which expreſſes the exceſs of Jupiter's equator above its axis, there is no neceſſity for the tranſlation of the arguments leading to a reſult ſo anſwering to the obſervations; and I paſs to the concluſion of that article, which ends my book.

The preceding theory agreeing with all the meaſures of the pendulum, and obſervations of Jupiter's diameters, if, beſides, it happen, that the meaſures expected from Peru give, when compared with thoſe of Lapland, a difference of diameters leſs than $\frac{1}{230}$, this theory will have all poſſible confirmation, and the univerſal gravitation ſo well agreeing with the motions of the planets will alſo agree with their figures.

Now I beg every candid reader to examine, whether, in that chapter quoted by F. Friſius, I have too much relied upon the certainty of obſervations, and attempted to diſparage Sir Iſaac Newton's diſcoveries.

In

In the firſt place, I will aſk of Father Friſius, if, before the operations, which I depended upon, were performed, I could eſtabliſh any thing againſt their agreeing, or not, with Sir Iſaac's propoſition about the ſame matter?

He perhaps will anſwer, That my remark of the LXIX art. *But if the diameters were found undoubtedly in a greater ratio to one another than* 230 *to* 231, imports, that I was not thoroughly convinced, that what care ſoever would be taken by the gentlemen ſent into Peru, they never would be able to meaſure their degree with a ſufficient exactneſs, to conclude, from its length, compared with that of the other degrees, whether the diameters were in a greater or leſs ratio than 230 to 231; and conſequently he will think, that my being in ſuſpenſe about it was an offence againſt Sir Iſaac's theoretical determination. Then, I requeſt Father Friſius to tell me, why he is ſo good as to commend operations ſo void of uſe as thoſe, which tended only to diſcover what was demonſtrated before, and needed not to be confirmed, ſince it could not be invalidated.

Perhaps Father Friſius, in repreſenting me as depending too much upon the obſervations, relied on theſe expreſſions of the LXIX art. *As the meaſures of the gravity cannot agree with the ſuppoſition of the homogeneity:* and I confeſs, that it ſeems to me impoſſible to reconcile the great number of all the meaſures of that ſort with the table, which follows the homogeneity. For the ſimplicity of the means made uſe of in the performance of thoſe meaſures cannot admit the errors, which ſhould be ſuppoſed to bring them to Sir Iſaac's theory: but as this theory is founded

on

on the homogeneity, which is only a mere ſuppoſition; and as he has himſelf ſuſpected, in his ſecond and third edition, that the internal parts of the earth might be denſer than thoſe towards the ſuperficies, I do not ſee how I oppoſe myſelf that illuſtrious philoſopher, when I aſſume the ſame hypotheſis, as he does. As I ſhall uſe all poſſible endeavour to underſtand F. Friſius's meaning, I hazard this conjecture. Seeing that I thought favourably enough of the exactneſs to be obtained in aſtronomy, when obſervations have been already made in great numbers, and with all poſſible care, to ſuppoſe them fit to let us know, whether the diameters are in a greater or a leſs ratio than 230 to 231; and being informed afterwards, that the operation made in Peru led thoſe, who have made uſe of it, to imagine the ſpheroid flatter than the homogeneous, he concludes, that I cannot help thinking like them, and accordingly indulges himſelf in expoſing, how much I over-rate the validity of obſervations, and how little I know the ſubmiſſion due to a propoſition of Sir Iſaac; which, I muſt ſay, by the bye, that great man has never himſelf given as impoſſible to be oppoſed by experience. But yet I would aſk of F. Friſius, wherefore he will gueſs at my ſentiments, whilſt I have not given room to know them on that point? How can he know, whether, ſince the examination of all the meaſures, I have not found any way to reconcile them with the theory? Which I ſay in no manner as a hint I intend to make any corrections in thoſe meaſures, but merely to ſhew the little foundation, which F. Friſius had to repreſent me as he has done.

However difficult it may be to account for Father Frisius's expressions, I shall hazard yet another conjecture. His great zeal for Sir Isaac, for which he is certainly to be commended (if not blinded by that zeal) has hindered him from distinguishing between the different ways of opposing that great man's sentiments. Perceiving then, that my calculations (§ L. Part II.) had led me to a result quite different from Sir Isaac's assertion, (Prop. XX. lib. 3.) he was offended at my boldness to such a degree, that he was unable to examine impartially what I said; and, instead of discussing a mathematical question quite independent of any actual measure, wherein if I were mistaken, he would have forced every geometrician to condemn me, he has supposed, that I have built my argument upon an operation, which was not performed at the time when I wrote.

This conjecture would appear to me the true cause of F. Frisius's error, if it were not inconsistent with a proceeding of his towards Sir Isaac, which I will venture to relate. After F. Frisius has examined himself the 19 problem of the third book of the *Principia*, which is much less complicated than that I spoke of, the truth of which is incontestable, he finds, by his own mistake, a disagreement with the result of that proposition, and charges that illustrious author, without the least apology, with an error, which, says he, (quite from the purpose) is the sixth, that has been found in the same work, and also gives an enumeration of the five others, altho' they are not at all concerned in the question.

I cannot forbear saying, that the manner, in which I have proposed my remarks upon the 20th proposition

of Sir Iſaac, has nothing of that ſlight way of treating ſo great a man; and as my utmoſt wiſh is to be judged on that account by the Royal Society, I ſhall relate what were my objections; which I cannot effect in a more conciſe and clear method, than by giving the tranſlation of the article, which contains it.

§ LI. of the ſecond part of the theory, &c.
" *In which is ſeen what had induced Sir* Iſ. Newton
" *to think, that the planets, when denſer at the cen-*
" *ter than at the ſurface, ought to be flatter than in*
" *caſe of homogeneity.*

" Some years ago I gave, in the *Philoſ. Tranſ.* " N° 449, the theorem of the precedent article; " and on this occaſion I mentioned a paſſage of Sir " Iſaac contrary to it. Not having at that time " looked into the ſecond edition of his *Principia*, I " could not know what had engaged that illuſtrious " philoſopher to think ſo; and far from ſuſpecting " any miſtake in his propoſition, I was contented to " think, that the difference between our concluſions " aroſe from a different way of conceiving the inſide " of the earth; and I imagined, that he had happen'd " to fall upon ſuch a diſpoſition of parts, as would " anſwer to his aſſertion. I then follow'd only his " commentators, and eſpecially Dr. Gregory, ſhew- " ing, that his explanation of Sir Iſaac's concluſion " was wrong, as grounded upon a propoſition, which " did not hold in the preſent caſe For that pro- " poſition (which is, that the gravity at any point " of the earth is inverſedly as the diſtance from the " center) has only room, when the earth is homo- " geneous; and, conſequently, ought not to be made

" uſe of, when the denſity is greater towards the
" center than at the ſuperficies.

" Since I have diſcover'd, that the theorem, the " demonſtration of which I had given in the *Philoſ.* " *Tranſ.* for the caſe of beds ſuppoſed of the ſame " ellipticity, has room in an infinity of other ſuppo- " ſitions, I have taken greater care to diſcover what " could have induced Sir Iſaac to think, that the " earth is flatter, as the gravity is more decreaſing " from the pole towards the equator; and I believe " I have found it out in the ſecond edition of the " *Principia*, and it is, for having built upon the " ſame argument as Dr. Gregory.

" In p. 386, after having obſerved, that the ex- " periments gave a diminution of two lines to the " ſecond pendulum from the pole to the equator, he " argues thus: Since, ſays he, the caſe of homoge- " neity afforded only 1 $\frac{87}{1000}$ 4, the difference $7\frac{1}{6}$ miles " between the two diameters (which follow'd from " the ſame hypotheſis) is to be magnified in the ratio " of 1 $\frac{87}{1000}$ to 2, and it will come out 31 $\frac{7}{12}$ miles " for the real difference. For, purſues he, the re- " tardation of the pendulum at the equator denotes " the diminution of the gravity in that place; and the " lighter the matter is there, the higher will it riſe " to be equiponderant with that of the pole.

" Further, *p.* 387, examining the meaſures of the " degrees of latitude made in France by M. Caſſini, " by which the earth is higher at the pole than " at the equator by about 95 miles, he pretends, " that accordingly the pendulum ſhould be longer at " the equator than at the pole by about half an inch. " And all that, methinks, ſhews the opinion, which

" Sir

" Sir Isaac was of, that, in any case whatsoever, the " equilibrium requires a gravity inversedly proportional to the length of the columns, which proportion, as I hope to have evinced, is only demonstrated in the case of homogeneity, and is not true " in general. Thus, what I argued in the *Phil. Transf.* against Dr. Gregory, has also room against " Sir Isaac."

By all that I have said, every body may judge, whether differing from Sir Isaac's sentiments on a point, which I had for so long a time examined, I did not express my disagreement with him in as decent a manner, as any one should, when speaking of so great a man. And in case the Royal Society thought some alterations were to be made in the form of my remarks, I declare, that I shall execute it, as may be prescribed to me by that illustrious company. But I cannot help thinking, that, unless those, who would examine my demonstrations, find some error in them, no alteration is requisite to be made in my expressions. I desire then either F. Frisius, or any geometrician, who thinks the question worth his examination, to take the trouble of reviewing my calculations, and to believe me ready to acknowlege my error, when shewn to me by a candid and impartial examiner.

XI. *An Account of a Storm of* Thunder *and* Lightning, *near* Ludgvan *in* Cornwall, *in a Letter from the Rev. Mr.* Wm. Borlaſe, *M. A. F. R. S. to the Rev. Dr.* Lyttelton, *Dean of* Exeter.

Revd Sir, Ludgvan, Feb. 1, 1753.

Read Feb. 15, 1753.

SINCE you have received only a ſhort and general account of the lightning in my neighbourhood on the 20 of December laſt, I ſend you the notes, which I took on the ſpot; in which, you will excuſe me, altho' I ſhould be too minutely circumſtantial, for fear I ſhould omit any thing, which might contribute ever ſo little to give you, at this diſtance, a juſt idea of what happen'd.

About 8 that morning, I perceived the ſky all of a ſudden overcaſt in the eaſt with very dark and red angry clouds; and during the continuance of the clouds, the wind very boiſterous. Theſe clouds paſs'd away, and at intervals we had clear blue ſky, and then large clouds again, attended with cold ſhowers. About a quarter before twelve, there was neither rain nor wind, but ſunſhine. However, ſome flying thin clouds were at this time obſerv'd to join, and form a body in the north-weſt; and then enſued one of the moſt ſhocking peals of thunder I ever heard, attended here at Ludgvan with much lightning, but it did no harm. It was ſo alſo, in the high grounds of St. Hillary (next pariſh to this on the eaſt) where a gentleman travelling found all the downs round

him

him on a ſudden full of lightning, but was rather frighted than hurt. But this thunder-cloud broke about three miles to the weſt of this place, in the ſide of Moelfra hill, in the pariſh of Maddern, where the marks of it (when I trac'd them) were as in the following ſketches.

The firſt traces were an inciſion, or ſcratch, made in the turf, about three inches wide, and two deep, where the lightning coming up from the ſouth-weſt at *A*, paſſing through the bank *B*, and iſſuing out of the bank in three ſtreams, which united again at *C*, turn'd away to the north at *D*. About ten paces to the north of theſe breaches there are more marks of the ſame kind, but not in the ſame direction; for the lightning here came from the north-weſt, and, paſſing upwards, I obſerved the furrow, which it had made, to grow wider, and ſomewhat deeper, as it gain'd upon the hill, eſpecially where it met with bank or ſtone; and ſome banks were five feet wide, which had their tops untouch'd, but were pierced thro' as with a bullet. This ſecond furrow was (as all the reſt) not in a ſtrait line, but in a vermicular direction, and with its turnings led us to a Karn, or ledge of flat rocks, *E*. Here the lightning paſſing parallel to the ſide of the Karn, *H*, *L*, came in a direction from weſt by north, and meeting with a flat rock, *F*, *I*, (which in our country we call a quoit) ſtanding on its edge, that ſouthern part of which, *I*, directly oppoſed its paſſage, cut off all that end of this quoit at the dotted lines, ſplit the quoit into two thinner ones, took off ſeveral ſplinters, and left large ſpots of ſulphur (whitiſh like arſenic) on that ſide, whence the ſhivers were taken off. On the top of this quoit there is a remarkable inciſion,

incifion, about three inches long, and as many wide, from which a piece of that quoit, of the fame dimenfions, was carried off, and in the fame manner, as if a mufket-ball had been fir'd at it. The lightning from this rock fpread itfelf to the fouth, in two branches *G* and *M* breaking and rooting up fome ftones, and making its way clear under others, and appearing again on the other fide. Thefe laft-mentioned furrows were ten inches wide, and a foot deep; befides which, we found feveral places in the hill, which had holes about a foot wide, and 6 or 8 inches deep, and feveral clods cut thin and clear off from ground: which fhews, that as this lightning went like darts through banks and ftones, and tore up the ground in many places like a ploughfhare, fo in other places it fpread into an horizontal thin edge, which fcooped up and carried off the little unevenneffes of the turfy ground. The whole workings of this lightning were in length about a furlong from weft to eaft. There were feveral people not far from the hill at this time, whofe names I will not trouble you with. Two women, one half a mile, the other but a quarter of a mile diftant, faw a fmoke, at the Karn abovementioned, as if feveral mufkets had been difcharg'd there. A third woman, not a furlong diftant from the Karn, faw the town-place, where her houfe was, as it were all on fire; and during this dreadful thunder, the fheep on the hill ran to and fro, as if driven by a pack of dogs. This hill of Moelfra is the higheft land between north and fouth fea in this part, about two miles from the former, and four from the latter. The wind was at weft, and weft-north-weft.

This

The firſt thunder-clap was ſucceeded, in leſs than a quarter of an hour, by another, which broke at a village, in the pariſh of Gullval, called Trythal, about a mile and half to the ſouth-weſt of Moelfra hill, and was attended with the following melancholy accidents; which, that you may the better apprehend, I have added a plan of the houſe, ſhewing the different ſtations of the perſons, who ſuffered.

Thomas Olivey, a farmer of good ſubſtance and repute, was returned from the field, about a quarter before twelve o' clock, and had all his family round him in the kitchen, except his daughter, who was in the hall. There was a pan over the fire in the kitchen-chimney, full of boiling water. The farmer was ſitting by the fire (at 1), and his wife on a bench before it (at 2); their only ſon, 23 years of age, was ſtanding at the window (8), when it lighten'd much, and the firſt clap of thunder (ſpoken of before) follow'd. This clap of thunder was ſo violent here, that nothing was remember'd equal to it; and the back-door of the kitchen (6), which open'd to the north, quiver'd. The farmer called to his ſon, and deſired him not to ſtand ſo near the window, leſt the lightning ſhould hurt his eyes; upon which the young man removed from the window, backwards, into the corner of the room, and ſat down (at 3). For this, the apprentice-boy, laughing at him, was chid by his maſter, and luckily for him, ſent out of the room, to take care of ſomething without-doors. The lightning came from the weſt-north-weſt, and, falling upon the ſtack of the kitchen-chimney, which was about four feet ſquare, and as much in height, of hew'd ſtone, carried it

M clear

clear off from the houſe, and threw it into a pool of water twenty feet diſtant. In the chamber over the kitchen, directly beneath the top of the chimney, there was a little cloſet boarded in; all the boards were broken to pieces, the timbers of the roof ſhatter'd; alſo the bedſtead in that chamber; of the chamber-partition two planks were forced, a large cloaths-preſs thrown, and the ſouth windows of the chamber-floor (excepting one caſement) all broken, and blown out. From the top of the chimney, and chamber-floor, it deſcended into the kitchen below, where the family was: the farmer ſaw no lightning, nor heard any thunder, after the firſt clap before-mention'd; but, as ſoon as he had given the orders to his apprentice-boy, as before-ſaid, was ſtruck ſenſeleſs with the firſt flaſh, and thrown into the middle of the kitchen, and continued ſenſeleſs for a quarter of an hour. As ſoon as he came to himſelf, he aſked, who ſtruck him? but had not the uſe of his arms; and felt an aching pain, ſhooting, as he deſcribed it, into his bones; and a brand-iron, which hung in the chimney, being thrown down into the pan of water, had daſh'd the boiling water upon him to that degree, that his life was in extreme danger for more than a fortnight after.

Mrs. Olivey, ſitting at (2), was ſtruck down before the hearth (13). Both her ſhoes, tho' buckled on as uſual, were ſtruck off her feet; but her feet not hurt: and being neither burnt nor ſenſeleſs, was able to cry out for help, but could not move; for ſhe had no uſe of her under-limbs for a day and an half.

The

The farmer's brother was at the end of a long table (at 4) in the ſame room, and was only flung againſt the wall, about 3 feet diſtant, not hurt.

Mrs. Olivey's ſiſter was near the back-door (at 6); a plank of this door was ſtarted, and beat in: ſhe was ſtruck ſenſeleſs, and thrown twelve feet off againſt the ſettle (12), which ſtood againſt the ſouth wall of the houſe.

The farmer's ſon was ſitting (at 3); his coat and waiſtcoats (for he had two on) were torn into ſhreds, ſo that I could hardly diſtinguiſh where the pieces had formerly join'd; his ſhirt had a rent two feet long down the back, and was ſcorch'd; his left ſhoe torn from his foot; and the little toe of that foot ſo near cut off, that it hung but by a bit of ſkin; and he was quite dead. But, tho' reduced to this lamentable condition, as to his exterior, he was not mov'd from his ſeat, nor his face at all chang'd: his dog was lying at his feet, dead likewiſe, but never mov'd.

The farmer's daughter receiv'd the ſhock in the hall (at 7); was ſtruck ſenſeleſs, but revived ſoon; felt a trembling all over; her feet tickling, and partly benumb'd, and ſtiff, as if ſleeping; but perceiving in the room a cloud of ſmoke, and hearing her mother cry, ſhe made haſte into the kitchen, which ſhe found full of ſmoke, ſtinking like brimſtone. The lightning had left a mark quite croſs the clavel of the kitchen-chimney, about half an inch wide, in an undulating direction, broke thro' the partitions of the under-floor, thrown down the ſhelves, carried out all the ſouth windows, forced up the ſtair-caſe, blown out the north window (10), miſs'd or ſpar'd a clock, which ſtood cloſe by the window

(at 11); and being ſomewhat ſpent, when it reach'd the hall (7), carried out the windows; moved not ſome Delft baſons, which were in the ſouth window, but forced the door of a beaufet (14), at the end of the hall, an inch and a half inwards; and ſhook the eaſtern wall of the houſe to the very foundation.

I propoſed only to lay before you the matter of fact, in the order of time, place, and degree, as it happen'd; but I cannot help admiring the different motions, ſhapes, and effects, of this lightning.

The clouds over Moelfra hill, and the village of Trythâl (a ſpace of a mile and a half) were ſo heavily charg'd with lightning, that here they broke, both the firſt and the ſecond time, and the thunder-claps were within a few minutes of one another, as being produced but by two portions of one and the ſame congeries.

The general tendency of this lightning was as the direction of the wind at that time; that is, from the north-weſt to the eaſt, but where the principal exploſions were (as at the hill, and the houſe) many branches ſpread off in all directions.

Nor were the ſhapes, in which it operated, leſs different than its motions. Sometimes, as it appeared to me at Ludgvan, it was pointed as a dart; in ſome places edg'd as a ſcythe, now but one thin ſheet or ſtream, then two or three, and then one again. Now it fell as ſeveral ſeparate balls of fire; but upon the houſe as a large guſh, or torrent.

It was all fire, yet of different powers, according to the impregnation of its ſeveral portions. Subtil and penetrating as the electrical fire, it affected, ſhock'd, and permeated, all the human frame. Some

parts

parts of it only ſcorched wood, but did not melt iron, as with lightning is very common: ſome tore the leather and cloaths; ſome cut and wounded, and ſome killed without wound or rent; and other parts of this lightning again, upon ſtone, wood, leather, cloaths, and fleſh, only ruſh'd and forc'd with the power of air put into a violent agitation. All this happen'd in this place, and all in an inſtant: and altho' the cloaths were ſomewhat ſindg'd, as well as torn, and the young man's ſkin round his waiſt was alſo ſcorch'd, yet, from the general effects of this lightning in both places, I conclude, that it was rather ſwift, and irreſiſtibly piercing, than inflammatory.

The houſe ſtands very high, without tree or hill near it. I went thither purpoſely to examine into the circumſtances of this melancholy accident; and, after I had talk'd a little with the farmer and his family, and they had all (for my ſatisfaction) kindly enter'd into a detail of the particulars, the tears appeared in the eyes of ſome; others, even then, tho' almoſt a month after the misfortune, trembled; and all faulter'd in their ſpeech, and could not go on with their ſtory, without frequent pauſes. I remain,

S I R,

Ludgvan, Feb. 2, 1753.

Your moſt obedient ſervant,

Wm. Borlaſe.

XII. *A second Letter of the Rev.* William Henry, *D. D. to the right honorable the Lord* Cadogan, *F. R. S. concerning the* Copper Springs *in the County of* Wicklow *in* Ireland.

My Lord, Strabane, May 31, 1752.

Read Feb. 15, 1753

I RECEIVED this day the honour of your lordſhip's letter of the 18th, and muſt confeſs the great ſatisfaction, which I felt, in finding the account I ſent of the copper mines in the county of Wicklow, acceptable to your lordſhip.

It would be too preſuming in me, to think ſuch a rough account of theſe curioſities worthy of the attention of ſo great and learned a body as the Royal Society: but, as your lordſhip is the moſt competent judge, and the truly great and learned are better pleaſed with important diſcoveries in nature, than with fine poliſhing, I leave it to your lordſhip to communicate the account I ſent, to the Royal Society, in whatever manner you pleaſe. One thing I will pawn my reputation on, that every tittle in the account has the ſtricteſt truth to recommend it.

I did not ſee the iron ſhovel, which, by lying in the water, and being thereby incruſted with copper, gave the firſt occaſion to this important diſcovery; for this happened ſome time ago; and the ſhovel is long ſince gone. But I was informed on the ſpot of this incident by the miners and workmen, of whom there were a great number. It was afterwards confirmed to me by the managers and proprietors of the mines.

As

As to the other point of the iron bars impregnating the copper, in the manner defcribed in my letter, I examined it with the utmoft attention, and was an eye-witnefs to it in all its progrefs, and fo are thoufands. I faw the mafons laying a chain of new ftone troughs, or pits, for the copper-water to run through. I faw men laying the iron bars on wooden rafters in thefe troughs. I had the iron bars lifted up out of fome pits, where they had lain in the water from one to eight months, and faw them incrufted with the copper ruft; and corroded to thin plates, in proportion to the time they had lain in the water. I faw fome of the pits emptied, wherein the iron bars were wholly diffolved; and the labourers throwing up with fhovels the copper, which lay on the flags in the bottom of the pit, like mud: and out of one of the heaps of copper-mud, which I faw thrown up, I took that very parcel of copper duft, which I fent to your lordfhip. It was like mud, as it lay wet in the heap; but became duft, as it dried. I alfo faw feveral pieces of copper, which I was informed were made out of this kind of copper-mud. And Dr. Dumainbray, in his philofophical lectures in Dublin, fhew'd to us a plate of pure copper made in this manner; which led me to go to the mines, and there make this minute inquiry.

I muft take notice of one improvement, which I omitted in my letter. To prevent any dirt or mud from being carried out of the mines, by the ftreams, which are let into the pits, where the iron bars are depofited, the ftream, as it iffues out of the level, is collected into a large deep bafon, where all the dirt fub-

fides;

ſides; and the clear water only is from the ſurface of the baſon let out into the pits.

If there be any thing, wherein I may further ſatisfy your lordſhip's inquiries in this or any other matter, your commands ſhall moſt chearfully be obey'd by

Your lordſhip's much obliged, and

moſt obedient humble ſervant,

William Henry.

XIII. *The Conſtruction of the* logarithmic Lines *on the* Gunter's Scale; *by Mr.* John Robertſon, *F. R. S.*

Read June 18, 1752.

HAVING lately had occaſion to treat on the conſtruction of the Gunter's ſcale, I ſearched ſeveral books, wherein I ſuſpected were contained the reaſons of the common methods of laying down the logarithmic lines uſually put on thoſe ſcales: but not finding, either from my own ſearch, or that of my friends, any ſatisfactory account of this matter, I drew up the following paper, to be laid before the Royal Society.

The Gunter's ſcale * is an inſtrument almoſt univerſally known, and amply deſcribed by many writers; therefore

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* So called from its inventor Mr. Edmund Gunter, aſtronomy-profeſſor in Greſham-College, from March 6, 1619, till his death, Dec. 10, 1626.

therefore I ſhall not take up your time in uſeleſs repetitions, but only ſhew, on what principles the diviſions of the logarithmic ſines, tangents, and verſed ſines, are uſually protracted.

The line of numbers on theſe ſcales conſiſts of two equal lengths, commonly called two radii; the firſt containing the logarithms of numbers from 10 to 100; and in the ſecond are inſerted thoſe between 100 and 1000, or ſuch of them, as can conveniently be introduced.

Theſe diviſions are taken from a ſcale of equal parts; ſuch, that 100 make the length of one radius; and from this ſcale, the diviſions for the ſines, tangents, and verſed ſines, are alſo taken. Now, from this conſtruction of the line of numbers, it is plain, that, as the numbers in one radius exceed thoſe in the other, by one place in the ſcale of numeration; therefore the difference of their indices muſt alſo be unity: ſo that ſuch numbers only, whoſe index differs by 1, can be eſtimated in a length of two radii: but, in a length of three radii, numbers, whoſe indices differ by 2, may be read; and a difference of 3 may be reckon'd in a length of 4 radii, *&c.* The tables of logarithmic ſines, tangents, ſecants, and verſed ſines, are generally computed for a circle, whoſe radius is 10,000,000: therefore,

	°	'	''		°	'	''
In the ſines, the index 9 belongs to all between	90	0	0	and	5	44	36
The index 8	5	44	36	and	0	34	23
The index 7 to all between	0	34	23	and	0	3	27
6	0	3	27	and	0	0	21

&c.

	°	′		°	′	″
In the tangents, the index 9 belongs to all between	45	0	and	5	42	10
And the indices, 8, 7, 6, *&c.* fall as in the sines.						
In the versed sines, the index 10 belongs to all between	180	0	and	90	0	
9	90	0	and	25	51	
8	25	51	and	8	7	
7	8	7	and	2	34	
6	2	4	and	0	45	

&c.

Now, as the length of the Gunter's ſcale admits of no more than two radii, or of ſuch numbers only, whoſe index differs by unity; therefore, within this length, no more of the ſines, tangents, or verſed ſines, can be introduced, than thoſe, whoſe index differs by unity: And as not only the greateſt number among the ſines and tangents, but alſo thoſe more generally wanted, have the indices 9 and 8 differing by unity; therefore all the ſines from 90° to 0° 34′, and all the tangents from 45° to 0° 34′, are thoſe only, which are put on theſe ſcales; the diviſions anſwering to the leſſer ſines and tangents being omitted for want of room. And this is the reaſon, why the ſine of 90°, and the tangent of 45°, are limited by the ſame termination as the ſecond radius on the line of numbers.

To conſtruct the line of logarithmic ſines.

From the ſcale of equal parts, take the numbers expreſſing the arithmetical complements of the log. ſines

ſines of the ſucceſſive degrees, and parts of degrees, intended to be put on the ſcale, deſcending orderly from 90°: then theſe diſtances ſucceſſively laid from the mark repreſenting 90°, at the right-hand end of the ſcale, will give the ſeveral diviſions of a ſcale of logarithmic ſines.

For, the ends of any ſcale being aſſigned, the progreſſive diviſions of that ſcale are laid thereon from that end, which repreſents the beginning of the progreſſion: or, the ſame diviſions may be laid from the other end, by taking the complements of the terms to the whole length of the ſcale:

Conſequently the arithmetical complements of the ſines are to be laid from the diviſion repreſenting 90 degrees.

To conſtruct the line of logarithmic tangents.

Theſe are laid down in the ſame manner, and for the ſame reaſons, that the ſines were; the tangent of 45° ſtanding againſt the ſine of 90°.

The diviſions for the tangents above 45°, are reckoned on the ſame line from 45° towards the left-hand; or any tangent and its co-tangent are expreſſed by the ſame diviſion.

Thus one mark ſerves for 40° and 50°; and the diviſion at 30° ſerves alſo for 60°; that at 20° ſerves for 70°, *&c.* and the like is to be underſtood of the intermediate diviſions.

For, as the tangent of an arc, is to radius;
So is radius, to the co-tangent of that arc.

Therefore the tangent is equal to the ſquare of radius divided by the co-tangent.

And the co-tangent is equal to the ſquare of radius divided by the tangent.

Now the radius being unity, its ſquare is alſo unity.

Therefore the tangent and co-tangent of any arc are the reciprocals one of the other.

But the reciprocals of numbers are correlatives to the arithmetical complements of their logarithms.

Therefore the logarithms of a tangent and its co-tangent are arithmetical complements one of the other; and conſequently will fall at equal diſtances from 45 degrees.

Therefore, in the line of logarithmic tangents, the diviſions to degrees under 45 ſerve alſo for thoſe above; both being equally diſtant from 45 degrees.

To conſtruct the line of logarithmic verſed ſines.

As the greateſt number of degrees will fall within the limits of the ſcale by beginning at 180°; therefore the termination of this line is at 180°, which is put againſt 90° on the ſines: and altho' the numbers annexed to the diviſions increaſe in the order from right to left, yet they are only the ſupplements of the verſed ſines themſelves.

Now ſubtract the logarithmic verſed ſines of ſuch degrees and parts of degrees as are intended to be put on the ſcale, from the logarithm verſed ſine of 180°; then the remainders taken from the foreſaid ſcale of equal parts, and laid ſucceſſively from the termination of this line, will give the ſeveral diviſions ſought.

The following table to every 10 degrees was conſtructed in the foregoing manner, and are the numbers

to

to be taken from the ſcale of equal parts, for the degrees they ſtand againſt.

Degrees	Supplements of Verſed Sines	Degrees	Supplements of Verſed Sines	Degrees	Supplements of Verſed Sines
180	0,00000	120	0,[illegible]2[illegible]94	60	0,[illegible]0206
170	0,00331	110	0,173[illegible]7	50	0 74810
160	0,01330	100	0,23149	40	0,93190
150	0,03011	90	0,30103	30	1,17401
140	0,05403	80	0,38387	20	1,52066
130	0,08545	70	0,48282	[illegible]	2,2[illegible]941

From this table it appears, that the leaſt verſed ſine, which can be introduced within the length of a double radius, falls between 10° and 20°, where the index changes from 1 to 2; which will happen about $11^{\circ}\ 28'$.

If a table of logarithm verſed ſines to 180° are wanting, they are eaſily made by the following rule.

> Take the logarithm ſine of 30 degrees from twice the logarithm ſine of (N) any number of degrees; the remainder is the logarithm verſed ſine of ($2N$, or) twice thoſe degrees."

For it is a well-known goniometrical property, that the ſine of any arc (A) is a mean proportional between radius (R) and half the verſed ſine of twice that arc.

Therefore, putting v for the verſed ſine, and s for the ſine;

The

The $v\,2\,A = \left(\frac{2\,ss\,A}{R} = ss\,A \times \frac{2}{R} = ss\,A \times \frac{2}{10} = \right)$ $ss\,A \times \frac{1}{5}$; radius being 10.

Or the log. $v\,2\,A = 2$ log. $s\,A$ — log. 5.

But when radius is 10, the fine of 30° is 5.

Therefore the log. $v\,2\,A = 2$ log. $s\,A$ — log. fine of 30°.

Moft of the writers on this fubject give the following rule for laying down the divifions of this line:

> From the line of logarithmic fines, take the diftance between 90° and any arc; that diftance being twice repeated, from the termination of the line of verfed fines, will give the divifion for twice the complement of that arc."

Thus the diftance between 90° and 20° on the fines twice repeated, gives the verfed fine of 140°; or twice 70°, the complement of 20°. For the divifions, to be laid on this line, are the differences between the logarithm verfed fine of 180°, and the logarithm verfed fines of the fucceffive arcs.

Now the difference between the logarithm verfed fines of 180°, and of any arc $2\,A$, is log. ver. fine 180 — 2 log. fin. A + log. fin. of 30°.

Or, 10,30103 + 9,69897 — twice log. fin. of A.

Or, 20,00000 — twice logarithm fine of A.

Or the arithmetical complement of twice logarithm fine of A.

That is, the difference between the logarithm verfed fine of 180°, and the logarithm verfed fine of any arc, is equal to double the arithmetical complement of

of the logarithm ſinc of half that arc, rejecting the indices.

But, as theſe differences give the diviſions to the ſupplements of the real verſed ſines; therefore the arithmetical complement of the logarithm ſine of any arc being doubled, will give the diſtance of the diviſion for the ſupplement of twice that arc on the line of verſed ſines.

Thus, for 70°, the logarithm ſine is	9,97299
The arithmetical complement is	0,02701
Its double is	0,05402

Which is the number in the foregoing table ſtanding againſt 140°, and is the ſupplement verſed ſine of twice 70 degrees.

Now, as the arithmetical complement of the log. ſines of arcs, are the diſtances on the line of ſines between 90°, and the diviſions to thoſe arcs; therefore the diſtances between 90° and any arc, being twice repeated, will give the diviſion of the ſupplemental verſed ſine to twice the co-ſine of that arc.

XIV. *A Letter from Mr.* John Dollond *to Mr.* James Short, *F. R. S. concerning an Improvement of* refracting Teleſcopes.

S I R,

Read March 1, 1753.

IT is well known, that the perfection of refracting teleſcopes is very much limited by the aberration of the rays of light from the geometrical focus; which ariſes from two very different cauſes; that is, from different degrees

grees of refrangibility of light, and from the figure of the ſphere, which is not of a proper curvature for collecting the rays in a ſingle point. The object-glaſs is chiefly affected by the firſt of theſe; nor has there been yet any method diſcover'd for rectifying that aberration ſo, as in the leaſt to remove the indiſtinctneſs of the image ariſing from it. We are therefore reduced to the neceſſity of contracting their apertures, which renders it impoſſible to magnify much without very long glaſſes.

But the caſe is widely different with regard to the eye-glaſſes; for, tho' they are very much affected by both the aberrations before-mention'd, yet, by a proper combination of ſeveral together, their errors may be in a great meaſure corrected. If any one, for inſtance, would have the viſual angle of a teleſcope to contain 20 degrees, the extreme pencils of the field muſt be bent or refracted in an angle of 10 degrees; which, if it be performed by one eye-glaſs, will cauſe an aberration from the figure, in proportion to the cube of that angle: but if two glaſſes are ſo proportioned and ſituated, as that the refraction may be equally divided between them, they will each of them produce a refraction equal to half the required angle: and therefore the aberration being in proportion to the cube of half the angle taken twice over, will be but a fourth part of that, which is in proportion to the cube of the whole angle; becauſe twice the cube of one is but $\frac{1}{4}$ of the cube of two; ſo the aberration from the figure, where two eye-glaſſes are rightly proportion'd, is but a fourth of what muſt unavoidably be, where the whole is performed by a ſingle eye-glaſs. By the ſame way of reaſoning, when the refraction is divided

divided between three glaſſes, the aberration will be found to be but the ninth part of what would be produced from a ſingle glaſs; becauſe three times the cube of one is but one ninth of the cube of 3. Whence it appears, that, by increaſing the number of eye-glaſſes, the indiſtinctneſs, which is obſerved near the borders of the field of a teleſcope, may be very much diminiſhed, tho' not intirely taken away.

The method of correcting the errors ariſing from the different refrangibility of light is of a different conſideration from the former; for, whereas the errors from the figure can only be diminiſhed in a certain proportion to the number of glaſſes, in this they may be intirely corrected, by the addition of only one glaſs; as we find in the aſtronomical teleſcope, that two eye-glaſſes, rightly proportion'd, will cauſe the edges of objects to appear free from colours quite to the borders of the field. Alſo in the day-teleſcope, where no more than two eye-glaſſes are abſolutely neceſſary for erecting the object, we find, by the addition of a third rightly ſituated, that the colours, which would otherwiſe confuſe the image, are intirely removed: I ſay intirely removed; but this is to be underſtood with ſome limitation; for tho' the different colours, which the extreme pencils muſt neceſſarily be divided into by the edges of the eye-glaſſes, may in this manner be brought to the eye in a direction parallel to each other, ſo as, by the humours thereof, to be converg'd to a point in the retina; yet, if the glaſſes exceed a certain length, the colours may be ſpread too wide to be capable of being admitted thro' the pupil or aperture of the eye; which is the reaſon, that, in long teleſcopes, con-

ſtructed in the common manner, with three eye-glaſſes, the field is always very much contracted.

Theſe conſiderations, Sir, firſt ſet me on contriving, how to enlarge the field by increaſing the number of eye-glaſſes, without any hindrance to the diſtinctneſs or brightneſs of the image: And tho' others had been about the ſame work before, yet obſerving, that the five-glaſs teleſcopes, ſold in the ſhops, would admit of farther improvement, I endeavour'd to conſtruct one with the ſame number of glaſſes in a better manner; which ſo far anſwer'd my expectations, as to be allow'd by ſuch perſons, as are the beſt judges, to be a conſiderable improvement on the former.

Encouraged by this ſucceſs, I reſolved to try, if poſſibly I might gain ſome farther enlargement of the field by the addition of another glaſs: and by placing and proportioning the glaſſes in ſuch a manner, as to correct the aberrations as much as poſſible, without any detriment to the diſtinctneſs, I have obtained as large a field, as is convenient or neceſſary, and that even in the longeſt teleſcopes, that can be made.

Theſe teleſcopes with ſix glaſſes having been well received, and ſome of them being gone to foreign parts, it ſeems a proper time to ſettle the account of its origin; which is one of the motives, that has induced me to trouble you with this ſhort ſketch of the conſiderations, that gradually led me to its conſtruction; and I am emboldened, Sir, to write thus much, from the many favours I have already received at your hands, as well as from a ſenſe of your being a proper perſon to judge in ſuch caſes. And tho' I am ſenſible, that you are not unacquainted with the theory contain'd in this letter, yet foraſmuch

much as the ſubject has never been fully treated by any author, I ſhall endeavour, as ſoon as may be, to draw up a more particular explanation of the aberrations of light by refraction; but ſhall add no more at preſent, only beg leave to take this opportunity of ſubſcribing myſelf

Vine-Court, Feb. 21, 1753.

Your much obliged and

moſt humble ſervant,

John Dollond.

XVI. *A Comparison of different thermometrical Observations in* Sibiria; *by Mr.* Wm. Watson, *F. R. S.*

Read March 15, 1753.

ACCORDING to the thermometrical observations made by Monf. Demidoff, at Solifkamsky, on the borders of Sibiria, latit. 59, in the year 1751, the greateft degree of cold was upon Nov. 9, at 7 in the morning, when the thermometer, according to Fahrenheit's fcale ftood at 34 degrees below 0; which is 66 under the freezing point. This degree of cold, tho' much greater than what is ever obferved in thefe parts, is little, when compared with the accounts given us by Profeffor Gmelin, in the introduction to the *Flora Sibirica*. This gentleman, who was profeffor of chemiftry and natural hiftory at Peterfburg, was fent with feveral other learned men, to inquire into the natural hiftory of Sibiria, and was attended by fome ftudents, a painter or two, a miner, and other proper attendants. He continued nine whole years upon this expedition, and the obfervations he made, extraordinary as fome of them are, with relation to their truth, are fcarce to be doubted. The mercury in his thermometer, graduated according to De L'Ifle's fcale, often funk in winter, in very fouthern parts of this country, as near Selinga, in lat. 48, to near 226, which is equal to 55 and a half below 0 in Fahrenheit's thermometer, and is 87 and a half below his freezing point. But the cold is often more intenfe than this, as appears by the experiments made at Kirenginfhi,

			De l'Isle	Fahrenheit above 0	Fahrenheit below 0	Reaumur above 0	Reaumur below 0
January	highest	20 day 0½h p. m.	162	17,6			6,40
	lowest	31 day 7½ mane	200½		28,6		26,93
	at a medium		178⅓		2,0		15,11
February	highest	28 day 0½ p. m.	149	33,2		0,53	
	lowest	13 day 7½ mane	186		11,2		19,20
	at a medium		169½	8,6			10,40
March	highest	29 day 0½ p. m.	137	47,6		6,93	
	lowest	1 day 11 p. m.	167	11,6			9,07
	at a medium		149½	42,8		0,27	
April	highest	30 day 0½ p. m.	120	68,0		16,00	
	lowest	9 day 5½ mane	166	12,8			8,53
	at a medium		141	42,8		4,80	
May	highest	8 day 0½ p. m.; 28 day 0½ p. m.	118	70,4		17,07	
	lowest	1 day 4½ mane	157	23,6			3,73
	at a medium		135½	49,4		7.73	
June	highest	6 at 0½ p. m.	105	86,0		24,00	
	lowest	1 day 4 mane; 3 day 4 mane	143	40,4		3,73	
	at a medium		125	62.0		13,33	
July	highest	31 day 0½ p. m.	102	89,6		25,60	
	lowest	29 day 4 mane	146	36,8		2,13	
	at a medium		128¼	58,1		11,60	
August	highest	23 day 0½ p. m.	112	77,6		20,27	
	lowest	28 day 4½ mane	153	28,4			1,60
	at a medium		134⅓	50,8		8,35	
September	highest	1 day 0½ p. m.	124	63,2		13,87	
	lowest	18 day 5½ mane	155	26,0			2,67
	at a medium		139	45,2		5,87	
October	highest	10 day 11 mane; 11 day 0½ p. m.	143	40,4		3,73	
	lowest	25 day 6½ mane; 26 day 11 p. m.	169	9,2			10,13
	at a medium		154	27,2			2,13
November in the first 10 days	highest	1 day 0½ p. m.	152	29,6			1,07
	lowest	9 day 7 mane	205		34,0		29,33
	at a medium		179½		3,4		15,73
December	highest	16 day 0½ p. m.	150	32,0			
	lowest	20 day 8 mane	199		26,8		26,13
	at a medium		168¼	10,1			0,72

XVI.

Kirenginſhi, where its ſharpneſs was ſo great, that Profeſſor Gmelin with difficulty ſtaid at the door of his houſe, between three and four minutes.

Feb. 10, 1738, at 8 in the morning, the mercury ſtood at 240 degrees in De L'Iſle; which is 72 below 0 in Fahrenheit. At the ſame place in 1736, Dec. 11, at 3 *p. m.* 254 in De L'Iſle, almoſt 90 below 0 in Fahrenheit. Dec. 20, at 4 o' clock, *p. m.* 263 in De L'Iſle = 90 $\frac{44}{100}$ below 0 in Fahrenheit.

Jan. 9, 1735, 12 at noon, 275 = 113 $\frac{65}{100}$.

Jan. 6, 6 in the morning, 280 = 120 below 0 in Fahrenheit, and 152 below his freezing point.

Such an exceſs of cold could ſcarcely have been ſuppoſed to exiſt, had not theſe experiments demonſtrated the reality of it; and Profeſſor Gmelin aſſures us, they were made with all poſſible exactneſs, and agree with many others made in different parts of Sibiria by his direction.

It was not apprehended, that a greater degree of cold exiſted any-where, than that artificial one produced by Boerhaave, by means of ice and concentrated ſpirit of nitre, which ſunk the mercury 40 degrees below 0 in Fahrenheit's thermometer; and this was ſuppoſed to be the point, beyond which no animal could bear it: and Mr. Gmelin's relation is the more extraordinary, as the French academicians under the polar circle mention the greateſt degree of cold, obſerved by them, to be by Reaumur's thermometer 37 degrees, which nearly correſponds with 70 degrees below the 0 in Fahrenheit's.

XVII. *A Catalogue of the* Fifty Plants *from* Chelsea Garden, *presented to the* Royal Society, *by the worshipful Company of Apothecaries for the Year* 1752, *pursuant to the Direction of Sir* Hans Sloane, *Baronet, Med. Reg. & Soc. Reg. nuper Præses, by* John Wilmer, *M. D. Societat. Pharmaceut.* Lond. *Soc. Hort.* Chelf. *Præfect. et Prælect. Botanic.*

Read March 15, 1753.

1501 Acetosa Canopica minor. Lip. Jussieu.

1502 Alcea Virginiana, Ricini folio. Fl. Lugd. Bat. 2. 12.

Althæa Virginiana, Ricini folio. H. L. 23, Icon.

1503 Alysson incanum luteum, serpylli folio majus. Inst. R. H. 217. Thlaspi minus quibusdam, aliis Alysson minus. J. B. 2. 928. Clypeola Lin. Gen. 650.

1504 Anguria Americana fructu echinato eduli. Inst. R. H. 107.

Anguriæ folio, fructu, Ovi figura, et magnitudine, ad maturitatem pallido, spinosis tuberculis Momordicæ instar muricato. Pluk. Phyt. Tab. 170. f. 3.

1505 Astragalus orientalis, maximus, incanus, erectus, caule ab imo ad summum florido. Cor. Inst. R. H. 29.

1506 Ballote, folio Geranii Batrachoïdes. Gmel. Am. Ruth. 47.

1507 Bidens Americana, triphylla, Angelicæ folio, flore radiato. Plum.

1508 Blitum fragiferum majus vulgare; Spinachia fragifera. Ald. Hort. Farn. 85.

1509 Cakile maritima, ampliore folio. Cor. Inst. R. H. 49.
Cakile, seu Eruca marina latifolia. J. B. 2. 868.

1510 Caryophyllus sylvestris prolifer. C. B. P. 209. Armeria prolifera. Lob. Icon. 449.

1511 Cassida peregrina, Melissæ folio. Inst. R. H. 182.

1512 Caucalis major Daucoïdes Tingitana. Mor. Umbel. 65.

1513 Cistus femina, folio Salviæ, supina, humisparsa. C. B. P. 465.

1514 Cistus ladanifera Monspeliensium. C. B. P. 467.
Ladanifera, sive Ledon Monspessulanum, angusto folio, nigricans. J. B. 2. 10.

1515 Clinopodium Alpinum, roseum, Satureiæ folio. Bocc. Mus. 119.
Ziziphora. Lin. Gen. 28.

1516 Convolvulus, Linariæ folio, assurgens. Inst. R. H. 83.
Volvulus terrestris Dalechampii. Lugd. 1425.

1517 Coris cœrulea maritima. C. B. P. 280.
Coris Monspessulana cœrulea. J. B. 3. 434.

1518 Elichrysum orientale. C. B. P. 264. Stœchas citrina, floris et magnitudine et colore speciosa. J. B. 3. 154.

1519 Eryngium planum minus. C. B. P. 386.
Eryngium puſillum planum. J. B. 3. 87.

1520 Eupatorium, caule erecto, foliis cordatis, ſerratis. Lin. Hort. Cliff. 396.
Eupatorium, Urticæ foliis, Canadenſe, flore albo. H. L. 667.

1521 Hypecoum, tenuiore folio. Inſt. R. H. 230.
Hypecoum, ſiliquis pendulis teretibus æqualibus. Hort. Upſal. 31.

1522 Jacea Tartarica, laciniatis foliis. Gerber. Hort. Carolſt. 104.

1523 Jacea laciniata, flore luteo magno, ſquamis calycum ciliaribus, ſplendentibus. Gerber tanais. 172.

1524 Leucanthemum Alpin. Abſinthii folio. Boerh. Ind. alter 107.

1525 Leucoium, vernum, perenne, album, majus. Inſt. R. H. 221.
Heſperis Alpina, ſive muralis, minor repens. J. B. 2. 880.

1526 Lychnis ſylveſtris alba ſpica reflexa. Bot. Monſ. 171. Icon.

1527 Lychnis viſcoſa, foliis inferioribus Bellidi minori ſimilibus, flore minimo, carneo aut rubro. H. R. Monſp.

1528 Millefolium orientale, foliis Tanaceti incanis, radiis pallide luteis. Boerh. Ind. alt. 112.
Achillea, foliis pinnatis, foliolis lanceolatis, inciſis, ſerratis, ſubtus lanigeris. Lin. Hort. Cliff. 413.

1529 Moldavica, Betonicæ folio, floribus minimis pallide cœruleis. Amman. Ruth. 46.
Dracocephalum, floribus verticillatis, bracteis oblongis,

oblongis, integerrimis, corollis vix calycem æquantibus. Hort. Upſal. 167.

1530 Myoſotis incana repens anguſtifolia. N. D.

1531 Nepeta, floribus obliquis. Lin. Lophanthus. Amm.

1532 Orobus ſylvaticus, purpureus, vernus. C. B. P. 351.

Galega nemorenſis verna. J. B. 2. 343.

153 Phlomis, Hormini folio, floribus parvis ſuave-rubentibus villoſiſſimis. Amman. Ruth p. 39.

Galeopſis maxima foliis Hormini. Buxb. Cent. C. 1. p. 4.

1534 Ptarmica orientalis, foliis Tanaceti incanis, flore aureo. T. Cor.

1535 Ptarmica incana humilis, foliis laciniatis, Abſinthii æmulis. H. L. 510.

Abſinthium Alpinum umbelliferum, Cluſ. Hiſt. 340.

1536 Reſeda minor vulgaris. Inſt. R. H. 423.

Erucago Apula. Col. 269.

1537 Scandix Cretica minor. C. B. P. 152.

Aniſomaratrum. Col. 1. 90.

1538 Sclarea ſicula, folio argenteo ſubrotundo. Boerh. Ind. alt. 1. 163. Æthiopis tota argentea, perennis lanuginoſa. Cupan. H. Cath.

1538 Sclarea vulgaris lanuginoſa, ampliſſimo folio. Inſt. R. H. 179. Æthiopis. Dod. Pempt. 148.

1540 Scorzonera, foliis laciniatis, caule erecto. Inſt. R. H. 477.

Tragopogon, laciniatis foliis. Col. Phyt. 2. 21.

1541 Scrophularia Luſitanica fruteſcens, Verbenacæ foliis. Inſt. R. H. 167.

Scrophularia, foliis lanceolatis, obtusis, serrato-dentatis, pedunculis bifidis. Royen. Fl. Leyd. Prod. 294.

1542 Stœchas folio serrato. C. B. P. 216. Lavendula foliis crenatis. T. 198.

1543 Symphytum, consolida major, flore purpureo. C. B. P. 259.

Symphytum magnum. J. B. 3. 593.

1544 Thlaspi montanum sempervirens. C. B. P. 106. Iberis foliis linearibus acutis integerrimis. L. H. Clif. 330.

1545 Tragapogon hirsutum. C. B. P. 274.

Tragapogon Apulum, humile, hirsutum, luteum. Col. p. 1. 233.

1546 Valeriana maxima Pyrenaica, Cacaliæ folio. Fagon. Tourn. 131.

1547 Verbascum humile Alpinum, villosum, Borraginis flore et folio. Tourn. 147.

Sanicula Alpina, folio Borraginis, villosa. C. B. P. 243.

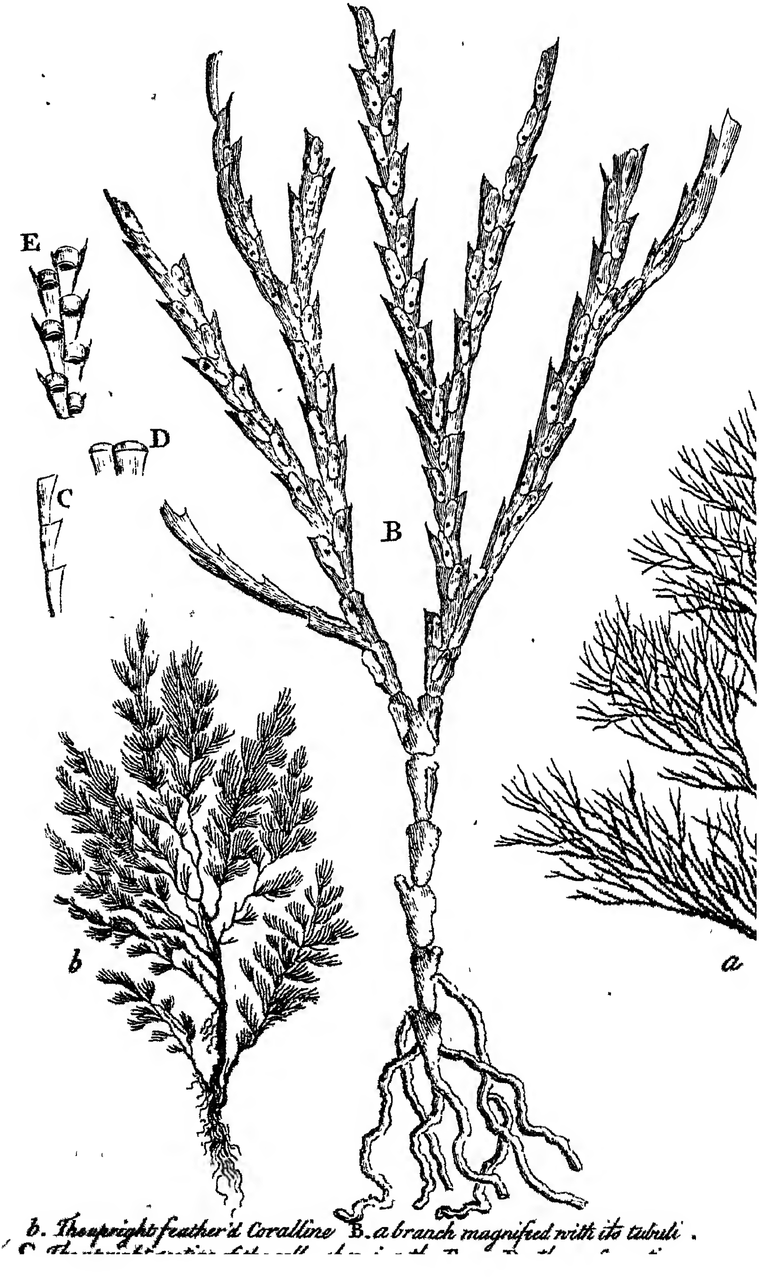
1548 Veronica Austriaca, foliis tenuissime laciniatis. T. 144.

Chamædrys spuria tenuissime laciniata. J. B. 3. 387.

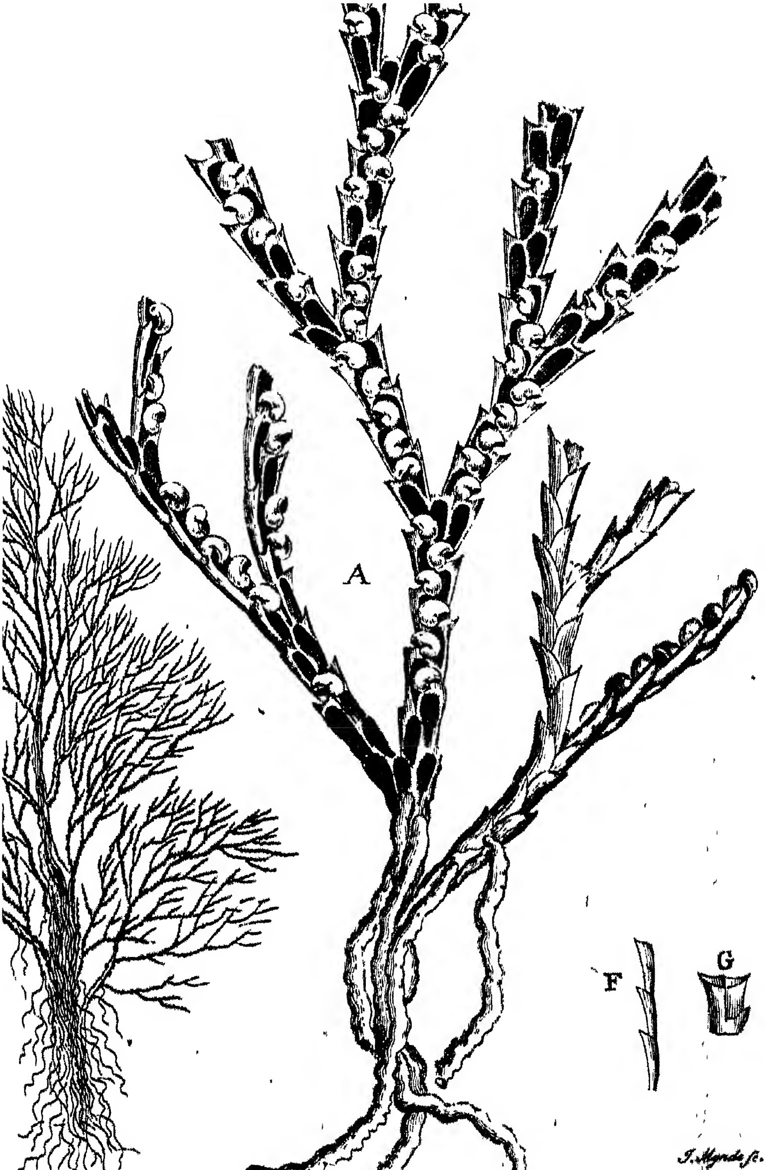
1549 Veronica orientalis, minima, foliis laciniatis. T. Cor. 7.

1550 Waltheria, foliis cordato-ovatis, serratis. L. H. Cliff. 342.

Monospermalthæa arborescens villosa, flore majore. D. Isnard. Act. Reg. Scient. 1721. p. 278, Tab. 14.



b. The upright feather'd Coralline B. a branch magnified with its tubuli.



a. the Snail bearing Coralline. A. a branch magnified. E. The Eggs turn'd to (testaceous animals
F. the upright section. G. the cross section.

XVIII. *Obſervations on a remarkable* Coralline, *in a Letter from Mr.* John Ellis *to the Rev.* Thomas Birch, *D. D. Secret. R. S.*

SIR,

Read March 17, 1753.

I BEG leave to preſent you with ſome obſervations, which I have made on a coralline, that I lately received from my curious and worthy friend Mr. Peter Collinſon. It appears, from its ſize and firmneſs, to belong to a warmer climate than this, and is probably American.

We find ſome of the ſame genus, but of a different ſpecies, of this coralline, on our own coaſts; but they are ſmaller, tenderer, and more tranſparent. There is one particularly, which comes very near this, called by Dr. Dillenius, in the third edition of *Ray*'s *Synopſis*, p. 37, N. 20, Tab. II. Fig. 1. *Corallina pumila erecta ramoſior:* and in *Buddle*'s *Hortus ſiccus*, in the late Sir Hans Sloane's collection, there is a ſpecimen like it, but not ſo fully advanced in its ramifications: this he calls *Fucus minimus hirſutus fibrillis herbaceis ſimilis*, from *Doody*'s *Appendix* to *Ray*'s *Synopſis*, p. 330.

This curious ſea production, which has the appearance of a plant, ariſes firſt from many ſmall vermicular wrinkled tubes, by which it appears to have adhered, like ours, to rocks, ſhells, fucus's, or other ſubmarine ſubſtances. Theſe tubes uniting form a ſort of ſtems, which, as they riſe, inſenſibly change into rows of cells: theſe ſtretch out into many regular

 dichotomous

dichotomous branches; each branch is made up of two rows of cells united together, and theſe cells placed in ſuch a manner ſide by ſide, that each cell joins two others on one ſide, and the bottom of one is inſerted in the top of the other. Their openings or faces look one way: they are nearly of an egg-ſhape, a little compreſs'd before: the broadeſt part is uppermoſt, and bends a little forward: the top of each is fortified by two angular points or ſpines.

By attentively viewing many ſpecimens of this genus of corallines in the microſcope, that have been taken out of the ſea at different ſeaſons of the year, I have obſerved the progreſs of nature to be pretty nearly thus:

The tubuli, or firſt beginning of the corallines in the younger ſtate, are found full of a yellow ſoft ſubſtance, which ſoon decays: in the more perfect ſtate they are clear and tranſparent.

The cells, which communicate with theſe tubes, have in the ſpring black ſpecks in each, which I take to be the embryo of the future production. During this very tender and minute ſtate, the opening of each cell is cover'd with an extreme fine tranſparent membrane, the uſe of which no doubt is to cheriſh and protect it.

Theſe ſpecks in time ſwelling into ſpherical teſtaceous bodies (as they are often found in ſummer) burſt through this membrane, and ſit in the front of the cell, ſupported by an umbilical ligament, which is faſten'd to the bottom of the inſide of each cell or matrix, till they come to maturity, which ſeems to be the caſe in the microſcopical drawing I have ſent you: wherein you will obſerve, they appear to be rows of very ſmall ſea ſnails, or rather teſtaceous bodies,

of

of the ſhape of a nautilus, ready to drop off, and provide for themſelves. In the ſame plate you have a microſcopical drawing of one of the Engliſh corallines of the ſame genus, with the embryo ſpecks in each cell.

I muſt further add, that I believe, if the curious, with good microſcopes, at the ſea-ſide, and at different ſeaſons of the year, would ſtrictly examine many of theſe beautiful ſea-productions, hitherto claim'd by the botaniſts, they would find, that ſeveral of the teſtaceous tribe proceed from ſome kinds of the larger corals, as well as, I am perſuaded, they will find, that many owe their original to the ſmaller corallines: and we are the more encouraged to try, ſince we obſerve, that various ſhapes and ſtages of the ſame animal are no new thing in the laws of nature.

S I R,

Your moſt obedient humble ſervant,

John Ellis.

XIX. *An Account of ſome uncommon* foſſil Bodies, *by Mr.* Henry Baker, *F. R. S.*

Read March 29, 1753.

THE foſſil bodies I have now the honour to lay before this Royal Society, I have never met with before, nor remember any deſcription of. They were ſent to me from Oxford, by Mr. William Frankcombe, a young gentleman reſiding there, who is very diligent in

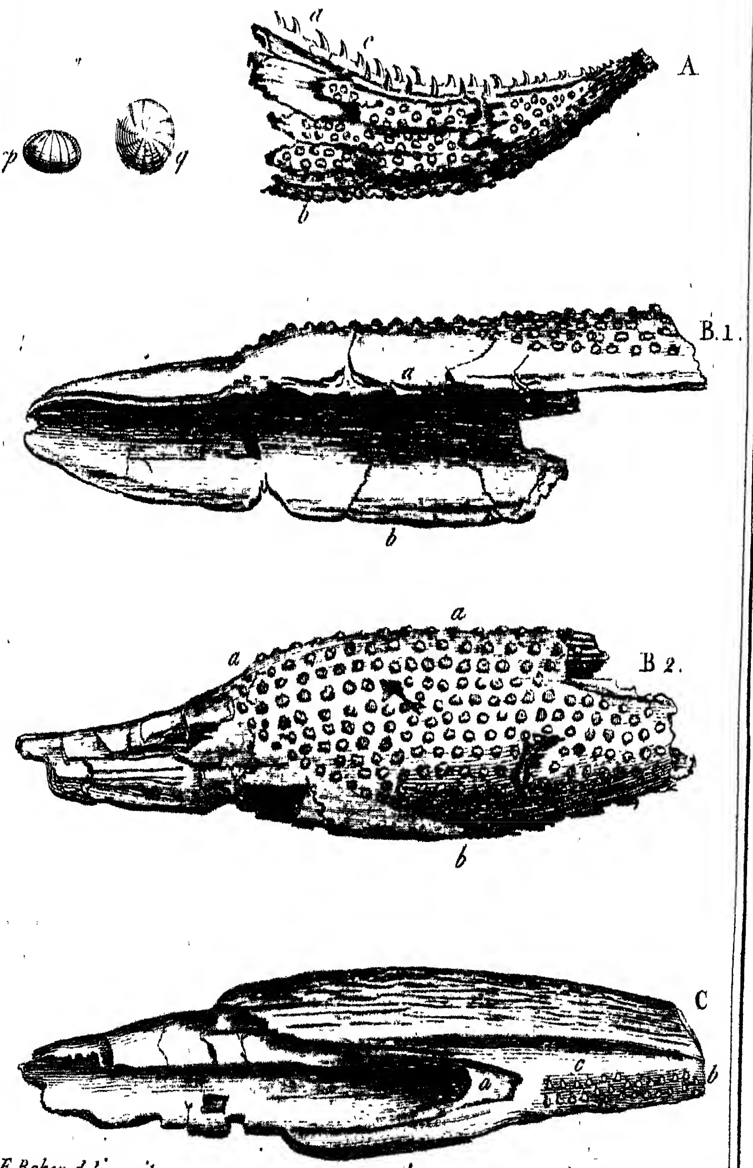
ſearching

ſearching after curioſities of this nature. He found them himſelf, but could not get them out of the bed they lay in without breaking them in many pieces: though he has glewed thoſe pieces ſo well together, that one may judge of them nearly as well as if they had not been broken.

As I muſt return them to Mr. Frankcombe when they have been examined by you, I have cauſed Drawings of them to be made, for the ſatisfaction of thoſe who may never have an opportunity of ſeeing them; to which Drawings I ſhall refer in the deſcription of them I am going to give. I ſhall then inform you, from his letter that accompanied them to me, where and amongſt what other kinds of foſſil bodies they were found, with other particulars relating to them. And afterwards I ſhall venture to lay before you a few conjectures concerning them.

Plate VI. ſhews theſe curious foſſil bodies at more than half their real bigneſs. They are only three in number, though there are four figures, one of them being drawn in two poſitions. They are evidently of a boney ſubſtance, made black, moſt likely, and rendered brittle, by ſome mineral ſteams or juices, though not corroded by them. Two of theſe bodies (*A* and *B*) have the greateſt part of their outer ſurface ſtudded, as it were, with pretty regular rows of tubercles, about the ſize of the heads of ſmall nails, riſing to a blunt roundiſh point, nearly one twelfth of an inch above the ſurface they iſſue from. Many of them appear radiated very prettily from the baſe to the apex; and perhaps they have all been ſo, tho' in ſome the lines are not now ſeen, and may have been

Philos. Trans. Vol. XLVIII TAB VI. *p. 118.*



D. E. Baker delineavit.

J. Mynde Sc.

been obliterated by time. Theſe tubercles are of a fine ſhining gloſſy black colour, and of a much cloſer and harder ſubſtance than the bone from which they riſe.

Figure *A* repreſents one of theſe foſſil bodies, whoſe length from end to end is ſeven inches and a quarter; on the ſides from *a* to *b* its breadth is two inches. The width of that part where the teeth are placed at *c* about ſeven eighths of an inch; but it gradually decreaſes, as does likewiſe the breadth of the ſides, towards the ſmaller end, which was probably about an inch longer than it now appears, and terminated in a point. The tubercles are largeſt in the broadeſt part, and the farther they are from the teeth, near which they are ſmall and flat: they likewiſe leſſen towards the ſmaller end, which is ridged for about an inch, and without any tubercles.

The under-part of this body is placed uppermoſt, for the ſake of ſhewing its teeth to the beſt advantage. There are two rows, running longitudinally, on a little riſing in the middle, with no great regularity, and ending in one row of very ſmall ones. The largeſt are about a quarter of an inch in length, hooked, of a ſhining black colour, having ſtill the natural poliſh, and being extremely ſharp and perfect. The ſides of this foſſil have ſwelled out, and been naturally more rounded than they are at preſent: for they plainly appear to have been cruſhed and compreſſed together by ſome foreign force.

B 1, is a foſſil body, ten inches in length, one part of which is rounded, and the oppoſite part hollowed: this figure ſhews the hollow part, which from *a* to *b* is more than one inch and half over; the

chane

chanel runs its whole length, and where deepeſt is an inch and half over, but it gradually grows ſhallower and narrower towards the ſmaller end. The ſides are a quarter of an inch in thickneſs.

B 2, ſhews the ſame foſſil body with the rounded part upwards. Its ſides from *a* to *b* are two inches. Great numbers of black ſhining tubercles, of the kind deſcribed Fig. *A*, but in general larger, and with leſs variation in their ſize as to one another, are diſpoſed in rows, pretty regularly in the manner ſhewn in the picture. Many of them appear ſtarry or radiated with ſeveral fine lines from the baſe to the apex, which lines riſe a little, and in ſome poſitions to the light appear of a whitiſh colour.

Two ſeparate figures of theſe tubercles are given (*p*, *q*) to make this account the better underſtood. One is a ſide and the other a front view. They are ſhewn magnified about eight times.

C, is a foſſil body, much more ſolid and weighty than the former two. Its length is ten inches. It is rounded on the upper part, where the ſides in the broadeſt place are one inch three quarters: the under part has a hollow or chanel one inch and an eighth in depth, ſeven inches and a half long, one inch and a half over, its bottom rounded. From *a* three inches and a half to *b* is quite ſolid, and at *a* in width one inch and a quarter, whence it goes tapering to *b*, where it is broken off ſo blunt as to ſhew, that it muſt probably have extended four or five inches farther. In this ſolid part *c* ſtand many ſmall teeth in rows, but not quite regular; ſome rows having but two, ſome three, and others four. They begin an inch diſtant from the chanel, and went probably to the extremity that is broken

broken off. They are black and ſhining like thoſe in *A*, but the points ſomewhat broken; tho' when whole they muſt have been leſs hooked, and much ſmaller than they.

The rounded part of this foſſil body has no tubercles like the other two, tho' it is plainly a ſpecies of the ſame kind with them, but is pretty ſtrongly furrowed, and the ridges have the ſame black gloſſy poliſh as their tubercles.

Mr. Frankcombe writes, " That he met with theſe " two bodies *A* and *B* in a pit, on the right-hand " ſide of the road, as you aſcend Shotover-hill from " Oxford. The uppermoſt ſtratum in this pit con- " ſiſts of a yellow ſandy earth; the next a browniſh " clay; then a regular ſtratum of large ſtony nodules, " about twelve inches thick; then a dark blue clay, " of about ten feet; and immediately under a rock of " free-ſtone. About two feet above the free-ſtone " were found the foſſils *A*, *B*. The firſt was found " at twice; the ſecond in ſearching to complete the " firſt, and both of them in many ſmall pieces, " as is evident from the bodies themſelves, which " he carefully joined with ſome thick gum-water. " That the firſt is of its proper ſhape and figure " plainly appears from the regularity of its tubercles: " and the ſecond is as he ſaw it himſelf in the ſtra- " tum. In this clay are found bones of ſeveral " kinds, oyſter-ſhells, *Ammonitæ*, cruſtaceous ſhells, " *Selenitæ*, and *Belemnitæ*.

" The cliffs on the right-hand ſide of Pyrton- " Paſſage over the Severn, Glouceſterſhire, afforded " the body marked *C*. This, ſays he, which was " likewiſe found in a ſtratum of blue clay, not unlike

" that at Shotover, and alſo in ſeveral pieces, appeared
" different from the others in nothing, but in the want
" of tubercles, and I flatter myſelf will ſerve to throw
" no ſmall light on the ſubject. His being not quite
" ſo converſant with theſe cliffs as with the pit at
" Shotover, prevents his ſpeaking of them ſo parti-
" cularly as he could wiſh."

The general appearance of theſe foſſil bodies gives reaſon to conjecture, that they are bones belonging to the head or ſnout of ſome animal of the fiſh-kind, or perhaps of ſome ſort of lizard, alligator, or crocodile.

The piece *A*, whoſe ſides are a little cruſhed, was found in Oxfordſhire, together with the piece *B* 1 and *B* 2, and may probably have been part of the ſame head: and if ſo, it ſhould ſeem from the two rows of teeth along its middle to have been the upper part of the head or ſnout: for ſome kinds of fiſhes have teeth in the palate or upper part of the mouth, but we know of none that have teeth along the middle of the lower part: there a tongue moſt commonly is placed, and the piece *B* has an hollow or chanel well adapted to contain a tongue. The teeth in the palate of the *lupus piſcis*, and likewiſe of ſome other fiſhes, are frequently found foſſil, of various ſizes and ſhapes, being what are called (very improperly) *Bufonitæ*. When the two pieces *A* and *B* are brought together, their ſize, figure, and appearance, greatly ſtrengthen the above conjecture: and 'tis worth obſerving, that the teeth are hooked inward, to prevent the prey when taken from eſcaping.

The piece *C*, found in Glouceſterſhire, ſerves likewiſe to confirm the ſame opinion: for the toothing in the middle thereof almoſt proves that part to have been

been the palate of ſome animal; an animal of the ſame genus too with *A* and *B*; tho' its having no tubercles, and being more ſolid, ſhew it to have been of ſome different ſpecies.

I remember not any foſſil bodies like theſe mentioned by authors, nor can I point out any animal, to which they may with certainty be imputed. Animal ſubſtances, before unknown, are met with frequently in the bowels of the earth: for the inhabitants of ſeas and rivers have been hitherto ſo imperfectly deſcribed, that we know but little of their internal ſtructure; and many ſorts we have never ſeen or heard of.— Amongſt the great numbers of foſſil ſhells we find, how few of their correſpondent kinds are ſeen in the beſt collections of recent ones? and what a variety of marine bodies, as well as parts of land-animals, are frequently dug up in this iſland, of kinds never found recent in our ſeas, or inhabitants of our clime, ſuch as *nautili*, *ammonitæ*, *&c.* the teeth and bones of elephants, the palmed horns of the elk, of the morſe-deer, and many others?

I ſhall be glad to have this ſubject conſidered by ſomebody more able to give you information; tho' no one wiſhes more the proſperity of this Royal Society, or can be with greater zeal,

Gentlemen,

Strand, March 29, 1753.

Your moſt obedient ſervant,

than

Henry Baker.

XX. *An abstract of a Discourse intitled,* The history of the emperor *Tetricus*, explained and illustrated by medals; *writen in French by Mr.* Claude Gros de Boze, *keeper of the medals in the French King's cabinet,* etc. *and sent by him to Dr.* Mead, *to be communicated to this Society. By* John Ward, *LL.D. Rhet. Prof. Gresh. and V.P.R.S.*

Read April 5, 1753.

AS the emperor *Tetricus* governed some years in *Gaul*, his reign does in a manner make part of the history of that country. But the accounts given of him by antient writers being very confused and imperfect, this learned and skilful antiquary has attempted to rectify and clear them up from medals. And as to those of *Tetricus* himself, the gold ones, as he observes, are in general exceeding scarce; and no medallion of this emperor in any metal was known, till very lately, when he procured one in gold for the French King's cabinet, a draught of which transmitted by him accompanies this paper (1). But tho Mr. *de Boze* professes only to give the *History of the emperor Tetricus*; yet such was the unsettled state of the Roman affairs at that time, as made it necessary for him to introduce it, by reciting a variety of incidents relating to other persons, which prepared the way for his advancement to that dignity.

He

(1) See Tab. II. Fig. 2.

He therefore begins his narrative with obſerving, that the *Gauls*, after they had been conquered by *Julius Caeſar*, continued pretty quiet under the Roman government, till the time of *Gallienus*; whoſe reign was ſo weak and ſcandalous, as to render him the common object both of hatred and contempt. *Valerian*, his father, had been created *Auguſtus* ſome years before his expedition againſt the *Perſians*; by whom he was defeated, made a priſoner, and treated with the utmoſt indignity (1). This event, which happened in the year 260 of the vulgar aera (2), aſtoniſhed the whole empire. And ſeveral nations, who were not concerned on either ſide, then offered their aſſiſtance to the *Romans*. Likewiſe divers princes in alliance with *Sapor* or *Sapores*, the Perſian king, adviſed him to make a good uſe of his ſucceſs, and ſecure to himſelf a ſolid and laſting peace, by releaſing his illuſtrious captive; but the haughty monarch would not hearken to any of thoſe ſalutary counſels.

Gallienus had the title of *Auguſtus* before that unhappy expedition, and being now at liberty to indulge his exceſſes without reſtraint, he ſhewed no concern for the misfortune of his father. Inſomuch, that upon the firſt news of it, affecting the air of a philoſopher, he ſaid, *Sciebam patrem meum eſſe mortalem.* And while a general greif ſpread itſelf among perſons of all ranks, he alone, as the hiſtorian expreſſes it, *Specie decoris, quod pater ejus virtutis ſtudio deceptus videretur,*

(1) *Trebell. Pollio, in* Valeriano. *Eutropius*, Lib. IX. c. 6. *Aurel. Victor*, *in* Epitome, cap. 32.
(2) V. Rom. 1013.

videretur, ſupra modum laetatus eſt (1). But altho this pretended philoſopher uſed no indeavours to procure his father's releaſe, he could not, as Mr. *de Boze* thinks, free himſelf from ſome remorſes of conſcience on that account; and therefore occaſioned a falſe report to be given out concerning his death, and divine honors to be confered on him, that he might be no longer talked of (2). The *Perſians* in the mean while, attentive to what paſſed at *Rome*, indeavoured to render *Valerian* daily a miſerable ſpectacle; ſtript of his imperial ornaments, loaded with chains, and expoſed to that contempt, that *Sapor* made a footſtool of his neck, whenever he mounted his horſe (3); in which deplorable ſtate he continued the remainder of his life.

As diſdain and reſentment often ſucceed murmurs, the barbarous nations eaſily threw off the yoke, which fear had impoſed on them; the moſt ſubmiſſive provinces thought it a diſgrace to obey *Gallienus* any longer; and the armies in moſt of the Roman territories choſe themſelves emperors, many of whom thro emulation ſoon deſtroyed one another. *Trebellius Pollio* has given a ſhort hiſtory of them in a continued ſeries; whom for the ſake of a round number he calls the *Thirty tyrants*, as not having been acknowledged by the ſenate.

The *Gauls* were in hopes, either not to have imbarked in this general conſpiracy, or however to have been

(1) *Trebell. Pollio*, Gallieni duo.
(2) *Ibidem.*
(3) *Lactant.* De morte perſecut. cap. 1. *Aurel. Victor*, Epitome, c. 32. *P. Oroſius*, LIB. VII. c. 22.

been the laſt in it. *Poſtumus* (1) had governed them almoſt three years by the appointment of *Valerian*; who writing to them in his favour, among other encomiums there given him, uſes this expreſſion, *Virum, quem ego prae caeteris ſtupeo* (2). He had before been governor to his ſon *Gallienus*, and prefered to *Aurelian*, who was afterwards emperor, on account of his mild and gentle diſpoſition; as *Valerian* himſelf intimates in a letter to *Antoninus Gallus* the conſul, where ſpeaking of *Aurelian* he ſais, that he was fearful, *Ne quid etiam erga filium ſeverius, ſi quid etiam ille feciſſet (ut eſt natura pronus ad ludicra) ſaevius, cogitaret* (3).

Gallienus was in *Gaul*, when he heard of the miſfortune, which had befallen his father; but departed ſoon after, leaving behind him *Saloninus* his elder ſon, whom he created *Caeſar*; and placed with him a tribune, by *Zoſimus* called *Silvanus* (4), but *Zonaras* calls him *Albanus* (5). This officer being jealous of *Poſtumus*, made it his buſineſs to diſguſt him, break his meaſures, and render him ſuſpected. Thus when *Poſtumus*, after an obſtinate ingagement, had intirely defeated ſeveral detachments of the *Bructeri* and *Chamavi*, people of *Germany*, who had paſſed the *Rhine*, and plundered the neighbouring country, he gave the ſpoil to his ſoldiers. But *Silvanus* wrote him a ſevere letter, and in the name of *Saloninus* ordered him to deliver

(1) M. CASSIVS LATIENVS POSTVMVS.
(2) *Treb. Pollio, in* Poſtumo.
(3) *Flav. Vopiſcus, in* Aureliano.
(4) Lib. 1.
(5) Tom. 1. pag. 632.

deliver up the whole booty, the diſpoſal of which was claimed by the prince. The army upon hearing of this order aſſembled in a tumultuous manner, declared both *Gallienus* and his ſon unworthy of the empire, tore off their images from the military enſigns, and obliged *Poſtumus* to aſſume the purple for the ſecurity of his own life (1). After this they marched immediately to *Cologn*, where *Saloninus* and *Silvanus* then reſided; and compelling the inhabitants to give them up, they maſſacred them both.

Gallienus, more concerned for the loſs of his ſon than his father's impriſonment, ſent no forces againſt the *Perſians*; but ordered his beſt troops to march into *Gaul*, with a ſhew of determined vengeance. *Poſtumus* oppoſed them for three whole years, at the end of which he was quite exhauſted, and obliged to defend his laſt town himſelf; when *Gallienus*, who was deſirous of the honour of conquering him in perſon, approaching too near the wall, was wounded in the ſhoulder with an arrow; which cauſed him to raiſe the ſeige, and return to *Rome*, without thinking any more either of *Poſtumus* or the *Gauls*. Who being thus freed from the danger which threatened them, juſtice, plenty, and the love of arts, flouriſhed among them under the government of *Poſtumus*; to whoſe honor divers monuments were erected, repreſenting as their tutelar deities, the godeſs *Pax*, but principally *Hercules*. And ſeveral of his medals are yet extant, with the title, RESTITVTOR GALLIARVM (2).

But

(1) *Zonaras*, ubi ſupra. *Treb. Pollio*, *in* Poſtumo.
(2) *Cabinet du Roy.*

But this felicity did not long continue; for the *Germans* and their confederates, whom he had defeated near *Cologn*, instigated by *Lollianus* (or rather *L. Aelianus*) who had been raised to oppose him, made a new eruption on the side of *Mayence*; where he again defeated them, drove them into the town, and so straitened them, that they offered to surrender. *Postumus* gave his consent, but the army would not; and rather than lose the plunder of a single town, sacrificed their general, whom they had themselves advanced to the imperial dignity.

Mr. *de Boze* places the death of *Postumus* in the spring of the year 267, being the seventh of his reign, and about the seventieth of his age. And his son of the same name, who is sometimes joined with him upon medals, appears to have lived near forty years. *Valerian* had given him the government of the *Vocontii*, a people in *Dauphiny*, at the same time that he sent his father to command in *Belgic Gaul*. And he was so considerable an orator, that some have ascribed to him the nineteen *Declamations*, which others assign to *Quintilian*, the grandfather of him, whose excellent treatise intitled, *Institutiones Oratoriae*, are yet preserved, and to which those *Declamations* are usually subjoined (1). *Trebellius Pollio* sais, that both the *Postumi* were killed in the sedition at *Mayence*. But Mr. *de Boze* doubts the truth of this, and thinks the credit of so inaccurate a historian, who mistakes even in their name, which he always writes

 Posthumius,

(1) *Treb. Pollio, in* Postumo juniore. *Berchii* Adverf. Lib. LVIII. c. 14. *Voss.* Instit. Orat. Lib. I. c. XI.

Posthumius, not sufficient to support it. He rather thinks therefore, that the son died some years before his father, and probably during the war, in which they were jointly ingaged against *Gallienus*. For it was immediately after this war, that his father took *Victorinus* (1) as a partner with himself in the empire; which, had his son been living, could neither have been for his interest, nor suitable to natural affection. Besides, tho *Victorinus* had then distinguished himself for his valour, he must have been younger than the son of *Postumus*; since his mother *Victorina*, or *Victoria*, was yet in the vigour of her age, and one of those who opposed *Gallienus*. And tho she could not possess the empire herself, such was her ambition, that she assumed the titles of *Augusta* and *Mater castrorum* (2).

Her son *Victorinus*, who expected to succeed to the government of *Postumus*, finding his army joined to the faction of *Aelianus*, and that he was proclaimed emperor, had no small difficulty to support himself among the other part of the *Gauls*, who by this means were divided. But this did not remain long; for *Aelianus* having undertaken to repair the forts, which *Postumus* built along the *Rhine* to prevent the passage of the barbarians over it; and they had

(1) M. AVRELIVS PIAVVONIVS VICTORINVS.

(2) The title *Augusta* was usually given to the mothers and wives of the emperors; and *Trajan* confered it likewise on *Marciana* his sister, and his neice *Matidia*. The other title, *Mater castrorum*, took its rise from the repeated acclamations of the armies; and had been given to *Faustina* the wife of *M. Aurelius*, and *Julia Domna* the wife of *Septimius Severus*, with some others.

had indeavoured to demolish, after they heard of his death; he imployed all his forces in that work, who displeased with the labour revolted and slew him (1). His death restored to *Victorinus*, what he had lost by that of *Postumus*, for he was acknowledged by all the *Gauls*. But he was soon after taken off by a private conspiracy, for an intrigue with the wife of a notary or clerk (2). His son also was killed with him, whom he had created *Caesar*, tho but an infant (3).

The army soon fixed on a successor, and imagining they should find better treatment from an inferior officer, with whom they had been more familiar, chose one *Marius*, an armorer by trade, who had raised himself among them, and was greatly admired for his intrepidity and bodily strength, qualities of the highest esteem with the vulgar. But *Marius*, thus advanced to the imperial dignity, immediately behaved with that cruelty and insolence, that one of his old freinds, trained up in his business, and afterwards his companion in the army, resenting his arrogant behaviour towards him after his advancement, stabbed him privately, with this opprobrious insult: *Hic est gladius, quem ipse fecisti* (4). His government was very short; *Aurelius Victor* (5) and *Eutropius* (6) make it no longer than two days, but *Trebellius*

(1) *Treb. Pollio, in* Lolliano.
(2) *Aurel. Victor*, De Caesar. c. 33.
(3) *Treb. Pollio, in* Victorino.
(4) *Idem, in* Mario.
(5) De Caesar. c. 3
(6) Lib. IX. c. 7.

Pollio ſais three (1). Tho Mr. *de Boze* juſtly obſerves, that neither of theſe accounts agrees with the number and variety of his medals, which ſeem to require more months; for they equal thoſe of his predeceſſor *Victorinus*, who reigned three years, from the time of his being an aſſociate with *Poſtumus* in the empire. Beſides, ſome of his medals appear to have been ſtruck on the account of victories gained by his lieutenants in that part of *Britain*, which was ſubject to the emperors of *Gaul*; the truth of which victories is confirmed by other monuments and inſcriptions found in that country (2), which ſeems no ways conſiſtent with ſo ſhort a reign. From whence he thinks it reaſonable to allow him four or five months, to be reckoned from September or October 267, to the end of January or February 268.

His ſucceſſor *Tetricus* (3), the principal ſubject of this diſcourſe, was a ſenator, had diſcharged the office of conſul, ſucceſſively governed almoſt all the *Gallic* provinces, and was then prefect of both the *Aquitains* (4). He was related to *Victorina*, who cauſed him to be declared emperor in his abſence, and the title of *Caeſar* to be given to his ſon. And being ſolemnly inveſted with the imperial dignity at *Bordeaux*, he was acknowledged, as *Poſtumus* had been before him, by all the *Gauls*, as likewiſe part of *Spain* and *Britain*. The time of his advancement

(1) *In* Mario.
(2) *Gul. Malmesburienſis*, De rebus geſtis Reg. Angl. Lib. II. *Camden.* Britannia, p. 641. edit. 1607.
(3) PVB. PIVESVS TETRICVS.
(4) *Treb. Pollio*, *in* Tetrico ſeniore.

ment to the empire Mr. *de Boze* places between the end of January and part of March, in the year 268 (1), agreably to the time he had affigned for the reign of *Marius*. For he obferves, that all hiftorians agree, that *Gallienus* was yet living, when *Tetricus* was declared emperor among the *Gauls*; and doubts only, whether he had heard the news, when he was killed before *Milan*, where *Aureolus* was befeiged by him. His death therefore muft have happened about the eighteenth or twentieth of March at furtheft that year. For his army having in the feild elected *Claudius* the fecond, afterwards furnamed *Gothicus*, of which he fent an account to the fenate, the letters arrived at *Rome* on the twenty-fourth; and the fenate being convened the fame day in the temple of *Apollo*, among other acclamations thefe following were feveral times repeated: *Claudi Augufte, tu nos ab Aureolo vindica. Claudi Augufte, tu nos a Palmyrenis vindica. Claudi Augufte, tu nos a Zenobia et a Victoria libera. Claudi Augufte, Tetricus nihil fuit*, or *nihil fit*, or *nihil fecit* (2), as the words are read in different manufcripts. From hence therefore he infers, that *Tetricus* muft at that time actually have held fomewhat, which could be nothing elfe but the empire of the *Gauls*.

The reign of *Claudius* was not long enough to anfwer all the hopes, which had been conceived of it. He marched immediately againft *Aureolus*, and defeated him. After this he purpofed to turn his arms againft *Zenobia*,

(1) V. Rom. 1021.
(2) *Treb. Pollio, in* Divo Claudio.

Zenobia, queen of *Palmyra*; but a deluge of *Goths* having on a ſudden overſpread *Illyrium*, *Thrace*, and *Macedonia*, he determined to repel them. And being obliged to take all his forces with him in that expedition, he not only forbore himſelf to diſturb *Tetricus* in his poſſeſſion of the *Gauls*; but likewiſe, as Mr. *de Boze* very probably conjectures, gave orders, that nothing might be done in his abſence to interrupt the peace, they had injoyed ſince the retreat of *Gallienus*. This real, or at leaſt feigned freindſhip, between theſe two emperors ſeems to be intimated by medals, ſtruck with the head of each of them on the oppoſite ſides (1). And the like, as he very ingeniouſly remarks, may be gathered from a paſſage of *Eumenius*, in his *Panegyric* upon the emperor *Conſtantine*, where addreſſing to him in behalf of the *Aedui* he ſais: *Divum Claudium, parentem tuum, ad recuperandas Gallias primi ſolicitaverunt; expectantesque ejus auxilium ſeptem menſibus clauſi, et omnia inopiae miſeranda perpeſſi, tum demum irrumpendas rebellibus Gallicanis portas reliquerunt, cum feſſi obſervare non poſſent* (2). Theſe *Aedui*, who applied to *Claudius* for his aſſiſtance, oppoſed at that time the government of *Tetricus*; whoſe medals give us a more noble idea of him, from the uſe he made of his victories. For in ſome of theſe he is repreſented not as a warrier, but in a ſtate of peace and plenty, with the legend SALVS AVGVSTORVM, intimating that moderation in ſucceſs is the true grandeur

(1) *Anſel. Bandur.* Numiſm. imper. Tom. 1. p. 403.
(2) Edit. ad uſum Delphin. pag. 222.

grandeur and ſafety of princes. And in others are ſeen the figures of ſeveral temples erected by him, ſome of them in a circular form like the *Pantheon*, with the legend PACI.

And to theſe happy times Mr. *de Boze* refers the curious gold medallion mentioned above, which repreſents *Tetricus* as crowned with laurel, and dreſſed in the *toga palmata*, or conſular robe, which was alſo worn in triumphs. In his right hand he holds an olive branch, and a ſcepter with the Roman eagle on the top in his left, and round his image is this inſcription, IMPERATOR TETRICVS AVGVSTVS. But the reverſe, if it has one, muſt remain unknown; ſince the medal is ſo fixed in the gold box, which contains it, that they cannot be ſeparated without indangering both. And the radiated circle of gold, with which it is incompaſſed, is deſigned only to adorn and inlarge it.

He therefore proceeds with the hiſtory, and further obſerves, that *Claudius* gained a moſt ſignal victory over the *Goths*, in which three hundred and twenty thouſand of them were ſlain, and two thouſand tranſport veſſels taken. But as this victory had been greatly facilitated by a peſtilence, which ſpread thro the enemies army, it ſeized likewiſe the Roman forces; and *Claudius* himſelf died of it at *Sirmium* in *Pannonia*, in the third year of his reign, and fifty-ſixth of his age (1).

His

(1) *Zoſimus*, Lib. 1. *Dexippus*, De legationibus. *Porphyrius*, in Vit. Plotini, Baſil. 1591

His brother *Quintillus*, who had been left with ſome forces in *Italy*, was proclaimed emperor there, and acknowledged by the ſenate. But being informed, that the victorious army had elected *Aurelian*, and perceiving that his own forces were inclined to abandon him, he choſe to free himſelf from thoſe unhappy circumſtances by a voluntary death (1).

Aurelian was not forgetful of the war, which his predeceſſor *Claudius* had deſigned againſt *Zenobia*, whoſe power daily grew more formidable, as ſhe had then got poſſeſſion of *Aegypt*. However the difficulty of the enterprize, and the great preparations neceſſary for undertaking it, occaſioned him to defer it near two years. But the vigorous manner, in which it was afterwards carried on by him, and the great ſucceſs that attended it, are too well known to need a recital here. In the mean while *Tetricus*, who remained unmoleſted, was conſtantly imployed in ſtudying the welfare and proſperity of the *Gauls*. And as both the ſituation, and natural fertility, of the country are very well ſuited to promote commerce; thoſe advantages were greatly improved by him, as well by repairing the roads, as making new ones, the overſight of which works was committed to his ſon; ſome proofs whereof yet appear from inſcriptions on the milliary pillars, erected to mark out the diſtance of the ways (2). The legends alſo upon ſome of their coins, ſtruck in honor of *Tetricus*, plainly

(1) *Zoſimus*, Lib. 1. *Zonaras*, Annal. Tom. 1. p. 636.

(2) One of theſe inſcriptions is yet preſerved at *Rouen*, in the houſe of Mr. *Bigot*.

plainly expreſs the happineſs, which the *Gauls* injoyed under his auſpicious government; ſuch as VBERTAS, LAETITIA, FELICITAS PVBLICA, and the like (1).

Victorina did not long ſurvive the advancement of *Tetricus* to the empire of the *Gauls*; but whether ſhe died a natural death, or was killed, hiſtorians have left quite uncertain (2). However, the arts of intriguing and caballing, which ſhe had carried to the greateſt hight in *Gaul*, did not die with her; but gave *Tetricus* a continual uneaſineſs, either to detect or ſuppreſs them. And therefore upon the return of *Aurelian* from the conqueſt of *Zenobia*, whom with her two ſons he ſent to *Rome* in great pomp; when *Tetricus* could no longer bear with the inſolence of his own ſoldiers, he wrote a letter to him, in which he uſed this expreſſion: *Eripe me his, invicte, malis* (3). And afterwards upon the arrival of *Aurelian* near *Chalons* in *Campania*, drawing out his forces, as if he deſigned an ingagement, he ſurrendered to him both himſelf and his whole army. By this means *Aurelian* being, then, as the hiſtorian expreſſes it, *princeps totius orbis* (4), celebrated a moſt ſplendid triumph at *Rome*; in which not only *Zenobia* with her two ſons, but likewiſe *Tetricus* and his ſon, were expoſed to public view among the other captives, to denote the ſubjection both of the eaſtern and weſtern empire.

Some

(1) *Cabinet du Roy.*
(2) *Treb. Pollio, in* Victorina.
(3) *Idem, in* Tetrico ſeniore. *Eutropius*, Lib. IX. c. 19.
(4) *Flav. Vopiſcus, in* Aureliano.

Some authors blame *Aurelian* for treating *Tetricus* in so indecent a manner; which however he is supposed to have done with a view to humble the senate, against whom he was greatly prejudiced; and therefore chose thus to show himself (as he was usually called by the populace) *senatorum paedagogum* (1). For *Trebellius Pollio* informs us, that he afterwards treated *Tetricus* with the highest honor, often calling him collegue, sometimes fellow soldier, and at other times giving him the title of emperor (2). His estate also was restored to him, and his house, which had been demolished, was rebuilt on mount *Coelius*, changed into a palace, and dedicated with solemnities like a temple. *Aurelian* was himself invited to this ceremony, and having entered the grand hall, was surprised to see himself represented there, as delivering to *Tetricus* and his son the senators robe with other marks of dignity, and receiving from them a civic crown and scepter (3). *Casaubon* finding that one of his manuscripts had not the word *sceptrum* in this passage of *Trebellius Pollio*, and not perceiving any reason for its being mentioned, proposes in his comment the omission of it. But Mr. *de Boze* very justly observes, that the gold medallion of *Tetricus* is the most certain commentary upon the place; where the scepter represents that of the Gallic empire, which compleated the glory and happiness of *Aurelian*.

Wherefore

(1) *Flav. Vopisc. in* Aureliano.
(2) *Treb. Pollio, in* Tetrico seniore.
(3) *Idem, in* Tetrico juniore.

Wherefore as he now thought himſelf in a condition to avenge the outrages, committed by the *Perſians* under *Sapor* upon the Roman empire, he entered upon that expedition; leaving the government of the greateſt part of *Italy* to the care of *Tetricus*, with this complaiſant expreſſion: *Sublimius habendum regere aliquam Italiae partem, quum trans Alpes regnare* (1). But *Aurelian* had marched no farther than *Caenophrurium* (a ſtation in *Thrace* between *Heraclea* and *Byzantium*) when he was killed by the treachery of his ſecretary, by ſome called *Mneſtheus* (2), and by others *Eros* (3). Who from a fear of puniſhment, for ſome miſdemeanours committed by him, had drawn up a liſt, containing the names of many perſons, ſome of whom had incurred the emperor's diſpleaſure, and others not; among which names, to prevent ſuſpicion, he had inſerted his own. This liſt, which appeared as writen with the emperor's own hand, he ſhewed to the ſeveral perſons named in it; adding withal, that *Aurelian*, whoſe temper was implacable, had determined to take them all off, if they did not prevent it in time. The ſcheme took effect, and *Aurelian* was aſſaſſinated in his march. But this treachery was not long concealed; and the army to avoid a freſh miſtake refered the choice of a new emperor to the ſenate, who returned them the compliment; and after ſeveral reciprocal meſſages of that ſort, which occaſioned an interregnum of ſeven or

(1) *Aurel. Victor*, *in* Epitome. *Treb. Pollio*, *in* Tetrico ſeniore. *Eutropius*, Lib. IX. c. 9.

(2) *Flav. Vopiſcus*, *in* Aureliano.

(3) *Zoſimus*, Lib. I.

or eight months, the ſenate elected *Tacitus* (1), one of their own body, and who was at that time *princeps ſenatus*.

No hiſtorian has ſettled the time, when *Tetricus* died. For *Zoſimus* only tells us, either from vulgar reports, or miſtaken memoirs, that he was put to death by *Aurelian* among other rebels in *Gaul* (2). All the reſt agree in ſaying, or ſuggeſting at leaſt, that he ſurvived his reſignation of that empire; which however could not be more than about eighteen months, or two years at the fartheſt, if he died before *Aurelian*. But that does not ſuit with the medals of *Tetricus*. For ſome of theſe have his head on one ſide, with a radiated crown; and on the other an eagle, funeral pile, or flaming altar; with the legend CONSECRATIO, the uſual emblem of deification. And as it can ſcarce be thought, that a prince of *Aurelian*'s martial diſpoſition would permit divine honors to be confered on one, whom he had himſelf led in triumph to mortify the ſenate; it is more natural to ſuppoſe, that it muſt have been done by *Tacitus*, who was raiſed to the empire by the ſenate, thought it his honor to govern by their counſels, and had nothing more at heart, than to repair the injuries they had ſuffered by his predeceſſors. And this opinion ſeems further confirmed by conſidering, that the ſucceſſors of *Tacitus* had a military turn, and copied after *Aurelian* both in their ſentiments and actions. As *Tacitus* therefore reigned but about ſeven months, from September

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(1) MARCVS CLAVDIVS TACITVS.
(2) Lib. I.

275 to ſome part of March 276, Mr. *de Boze* places the death of *Tetricus*, and his conſecration conſequent upon it, within that time, as what appears to him the moſt probable. And with that event he concludes his elaborate and curious diſcourſe.

G. C. March 21, 1753.

J. Ward.

XXI. *An Account of a Treatiſe, preſented to the* Royal Society, *intitled,* Flora Sibirica, ſive hiſtoria plantarum Sibiriæ tomus ſecundus, *extracted and tranſlated from the* Latin *of Profeſſor* Gmelin, *by* W. Watſon, *F. R. S.*

Read April 12, 1753.

THIS volume of the *Flora Sibirica*, now under conſideration, contains two hundred and forty pages in quarto, excluſive of the preface, and ninety-eight copper plates very curiouſly engraved. It was printed at Peterſburg in the year 1749.

An account of the firſt volume of this valuable work was communicated to the Society by my learned and ingenious friend Dr. John Fothergill *, and has been publiſhed in the *Philoſophical Tranſactions*: from its title, we are only promiſed an account of the plants of Sibiria; but Dr. John George Gmelin, its author at

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* See *Phil. Tranſ.* Vol. XLV, pag. 248.

at that time profeſſor of chemiſtry and natural hiſtory at Peterſburg, and now at Tubingen, went much farther, and has given us a very great number of new, curious, and uſeful obſervations, concerning the natural hiſtory of that vaſt and unknown region. The abundance of matter, and the limits of an extract, obliged Dr. Fothergill to confine himſelf, principally to the geographical and meteorological part of the work; but as the contents of the ſecond volume are chiefly botanical, I find myſelf obliged to take a review of the firſt volume, to introduce with propriety an account of the contents of the ſecond.

The *Flora Sibirica* contains the plants, which grow ſpontaneouſly in a region of vaſt extent, bounded by the Vralenſian mountains on the weſt, the ocean of Kamtſchatka on the eaſt, the *Mare glaciale* on the north, the countries of Kalmucks and Mongales, and the confines of China on the ſouth. Our author has, among the productions of theſe countries, interſperſed a few plants, collected by that excellent botaniſt Gerber, near the rivers Don and Wolga, and in the Ukraine; partly becauſe many of the ſame kind grow in Sibiria, and partly from a deſire that theſe curious plants ſhould no longer be concealed from the public. He has given no plant a place, which he himſelf hath not examined, at leaſt in a dried ſtate, and of which he is not ſatisfied of its generical character.

The plants of Kamtſchatka were collected by two of their company, detached for that purpoſe. Of theſe George William Steller is mentioned by our author, with very great reſpect, for his uncommon zeal and proficiency

proficiency in natural hiſtory; and for his offering himſelf to go upon an expedition, where he muſt for a long time, in very unhoſpitable regions, not only forego all the accommodations of human life, but be frequently liable to the miſeries ariſing from hunger, cold, and the barbarous inhabitants. Of theſe dangers, to uſe our author's own words, he was *contemptor ſtrenuus*, and continued ſeveral years upon the expedition, ſending from time to time to our author large collections and deſcriptions of ſuch natural bodies, as occurred to him. This excellent perſon, to the great grief of our author, and to the irretrievable loſs of natural hiſtory, died on his return home, after having ſurmounted almoſt incredible difficulties.

In digeſting the plants of the work before us into claſſes, our author has followed the method of our worthy brother Profeſſor Van Royen of Leyden, publiſhed in the *Flora Leydenſis prodromus*. This excellent botaniſt conſiders, that all plants may be ranged into twenty claſſes; and our author, in conſequence of this ſyſtem, has given five claſſes in his firſt volume, *viz.* thoſe which Van Royen intitles, *Palmæ*, *Lilia*, *Gramina*, *Amentaceæ*, and *Umbelliferæ*; and three claſſes in the ſecond volume, *viz.* *Compoſitæ*, *Aggregatæ*, and *Tricoccæ*: the twelve remaining claſſes therefore are, I preſume, to be publiſhed hereafter. Our author follows Van Royen's ſyſtem, not as he thinks it a perfect one, but as it nearly approaches to the order of nature; and has great relation to our countryman Mr. Ray's method, to which our author had been accuſtomed; and differs from it chiefly, inaſmuch as it is more complete. Some allowance muſt be made for cuſtom. He does not

detract from the methods of other authors; most of them please him; none of them are without their inconveniencies, and all have their advantages. The great point is, that the species should be well defined, and that each genus should have its essential character. Our author has generally adopted the genera of Linnæus; some indeed he has taken from Haller; but wherever he thought it expedient to differ from these great men, he gives his reason; and when he finds a plant, which cannot properly be ranged under any genus already established, he forms a new one; in the explanation of which, after the manner of Linnæus, he omits nothing essential thereto.

To the different species, discover'd in this expedition, P. Gmelin has affixed names, after the manner of Linnæus, Haller, Van Royen, and the more modern botanists, which are such, as that from the name of the species the plant may be known. But in what relates to the plants before discover'd, he adopts the names given them by the botanists just now mentioned, and scarce ever forms a new one; as he thinks a name already received, though but an indifferent one, should be retained, in preference perhaps to a better; lest the number of synonyms, already too great, should be augmented. To these he usually adds the synonyms of the Bauhins and Tournefort; and sometimes, for the sake of their figures, those of Morrison, Dodonæus, Plukenet, and Lœselius; and likewise those of the Russian botanists, Messerschmid, Bauxbaum, and Amman. He has also throughout the work carefully separated the varieties of plants from their genuine species, and has laid down the places of their growth, the names given them by the inhabitants,

and

and their application of them to the various purpofes of life. The figures of the plants were taken from the life, and are, as far as poffible, reprefented in their natural proportion; but from thefe muft be excepted thofe of Gerber, collected near the Don and the Wolga, and fome others collected by Dr. Lerche, phyfician to the Ruffian embaffy in Perfia, near Aftracan, and even in Perfia; thefe were delineated from dried fpecimens: and where-ever the figure does not, to our author's fatisfaction, reprefent the plant intended, by the neglect of the painter or engraver, he apprifes you thereof, and endeavours to remedy this defect in his defcriptions.

The great end of our knowlege in plants fhould be the inveftigation of their properties; and to this we are frequently obliged to be led, by the application of them among the people, where they are produced. In perufing therefore the *Flora Sibirica*, I have felected a few obfervations of this kind, which I think not improper to lay before the Society.

The venereal difeafe has made no inconfiderable progrefs among barbarous, as well as among the more polite and civilized nations; and our author has given us two methods of treating that diftemper among the inhabitants of Sibiria; from which, in fome degree, an idea may be formed of the ftate of medicine in thofe parts of the world. One method is, they give the patient a decoction of a fpecies of cirfium *, which

* " Cirfium inerme foliis fcabris, lanceolatis, inferioribus ex " finuato dentatis, fquamis fuperioribus calicum fubrotundis, mem- " branaceis." *Flor. Sibir. Tom.* II. *pag.* 72.

which grows in thoſe parts, and is deſcribed by nobody before our author: in this decoction, when the pains are violent, they add ſome leaves of a ſpecies of chamærhododendron, which produces effects ſimilar to opium, by relieving the pain, and ſometimes bringing on a delirium. If they are not cured by this decoction, which often happens in an aggravated ſtate of the diſeaſe, they then boil a ſmall quantity of ſublimate of mercury, with ſome fat, in a ſpoon over a candle, mix it with the before-mentioned decoction, and let the patient ſwallow it. It is no wonder, if, according to our author's relation, this rude method ſhould deſtroy the patient, and put an end to his life by ſevere torture, which frequently happens.

The other method of cure given us by our author is a more reaſonable one, and is effected by adminiſtring a cup-full or two of the decoction of a ſpecies of iris * every morning, detaining the patient in bed. Of this they give a greater or leſs doſe in proportion to its operation, which is both by vomit and ſtool. After having taken it a week, it ceaſes to have the effect of evacuating; nevertheleſs they continue it another week; during which time the patient is laid upon a heap of freſh burdock-leaves, and his body is alſo covered with theſe leaves, which muſt be renewed every day. This method is ſaid to cure the diſeaſe radically.

Ruſſians, Tartars, and other nations in theſe parts, eat as food, either boiled in milk, or roaſted in the embers,

* Iris foliis linearibus, corollis imberbibus, fructu trigono, caule tereti. *Lin. Hort. Cliffort.* p. 19. *Flor. Sibir.* Tom. I. p. 27.
Iris pratenſis anguſtifolia, non fætida, altior. *C. B. P.* p. 32.

embers, various ſpecies of the roots of lilies. The Tartars collect and dry the roots of the *dens canis* * of the botaniſts, and boil them either with milk or broth, and conſider them as very nutritious food. This root certainly is in every reſpect nearly related to Salep.

The Sibirian hunters, who kill various animals for their fur, are obliged to go in ſearch of them into the moſt deſart parts of the country, and remain there during their dreadful winters. It happens often, that, from the intenſeneſs of the cold, the leaven, which ferments their bread, is ſpoiled, and ceaſes to be of uſe. In this caſe they collect the inner bark of the larch-tree, which is very juicy and ſweet, and cut it into ſmall pieces, and digeſt it over the fire in warm water. They then add thereto ſome rye-flour, bury the whole in the ſnow, and let it remain there twelve hours; in which time the fermentation begins, and the fæces, which fall to the bottom, made excellent leaven.

Both the Ruſſians and the people of Kamtſchatka make great uſe of the § *ſphondylium vulgare hirſutum*, of Caſpar Bauhin and Tournefort; or, what we uſually call, cow-parſnep. According to our author, the plant in queſtion differs in nothing from that ſpecies, very frequently met with in the meadows and paſtures both of Germany and England, but in its being much larger. This difference of ſize the Ruſſian kind conſtantly preſerves,

* Erythronium. *Linnæi Hort. Cliff.* p. 119. *Flor. Sibiric.* Tom. I. p. 39.

§ Heracleum foliolis pinnatifidis. *Lin. Hort. Cliff.* p. 103. *Flor. Sibir.* Tom. I. p. 213. Sphondylium. *Rivin.* Tab. IV.

preſerves, when planted in the botanic garden. What we generally meet with here in England ſeldom grows higher than three feet; whereas the Sibirian plant is double that ſize. Our author has given us a very exact deſcription of it.

This plant, which has never yet been applied to any uſeful purpoſe in theſe parts of the world, is of very great importance to the Ruſſians and people of Kamtſchatka. They indeed apply it to very different uſes; the former diſtil their brandy * from it; the latter dry it to eat in winter. As theſe applications of this plant are, I believe, wholly new to us, and unobſerved by any preceding author, I ſhall lay before you a ſhort hiſtory of them.

About the beginning of July the radical leaves are arrived at their greateſt ſize and perfection, of which only the footſtalks are uſed; though, as far as may be judged from the ſmell, the ſtem of the plant is equally valuable. Theſe are ſtripped of their bark, and ſuſpended in the ſun in little bundles; and as they grow dry, many of theſe bundles are tied together, and expoſed again to the ſun, until they have parted with all their humidity. They are afterwards put up in bags, and in a very ſhort time are covered all over with a yellowiſh mealy ſaccharine exudation, of the flavour of liquorice; which, if it is wanted, is ſhaken off, and uſed as ſugar. The people of Kamtſchatka never ſeparate this ſubſtance from the ſtalks, but preſerve them together, and eat them themſelves, and regale their friends with them, as delicacies.

The

* Spiritum ardentem.

The Ruſſians dry them in the ſame manner, in order for diſtillation, and infuſe them in proper proportions with warm water, to which they add the berries of the * mountain dwarf-cherry, or thoſe of a ſpecies of *vaccinium* §, to promote fermentation. When this is over, they put both the ſtalks and the liquor, in which they have fermented, into a ſtill, and draw off the ſpirit as uſual. When the diſtillation is over, they do not throw away the ſtalks, until they have preſſed out their juicy liquor, which is added to freſh ſtalks to promote their fermentation.

From this ſpirit firſt drawn, they by diſtillation draw off ſomewhat leſs than half its quantity, which is very like to rectified ſpirit of wine, and much more pleaſant than corn-ſpirit.

It muſt be here obſerved, that, if either the ſtalks or leaves of this valuable plant are applied to the ſkin, they heat and ulcerate it. The people of Kamtſchatka however eat the crude ſtalks, when ſtripped of their bark, in which their acrimony conſiſts: But if, through ignorance, this bark is ſtripped off with their teeth, it inflames and veſicates their lips and gums, which will frequently continue a week before they are healed. In conſequence of this, ſome have made the experiment of extracting a ſpirit from the ſtalks without ſtripping them of their bark; and they have found, that they have furniſhed an equal quantity of ſpirit with thoſe, which have been ſtripped: But

* Chamæcerafus montana fructu ſingulari cœruleo. *C. B. P.* 451.

§ Vaccinia nigra fructu majore. *Parkins.* 1455. Vitis idæa magna quibuſdam. *J. B.* I. 518.

But it has been obſerved, that thoſe, who have drank of this ſpirit, have ſcarce eſcaped with life, and have complained violently of an oppreſſion about the præcordia a long time after.

From the mealy ſubſtance, which exudes from the ſtalks of this plant, a ſpirit may be prepared; provided that this ſubſtance is diluted in a proper quantity of water, and made to ferment: but this is in much leſs quantity than from the ſtalks themſelves. The fermented liquor likewiſe they uſe as wine, and frequently intoxicate themſelves therewith. By what accident it was diſcovered, that this plant would by diſtillation furniſh an inflammable ſpirit, and for many particulars relating thereto, I muſt refer you to the work itſelf; and our author informs us, that the ſeveral uſes of this plant were collected with great labour by M. Kraſkeninikoff, one of their company, from the Coſſacks of Kamtſchatka.

Dodonæus * relates, that the inhabitants of Poland and Lithuania make themſelves a kind of liquor, which the poor people uſe as beer, from the fermented leaves and ſeeds of the *Sphondylium*. This adds ſomewhat to the credibility of the former relation.

When Steller, whom our author always mentions with great eſteem, was at Tobolſki in the year 1738, he was informed, that two years before they were grievouſly afflicted there with peſtilential carbuncles, which were of ſo contagious a nature, as to ſeize thoſe, who approached the perſon affected. The diſeaſe firſt began in horſes and oxen, and afterwards ſeized

* Dodon. Stirp. Hiſtor. p. 304.

ſeized the human ſpecies. A red ſpot firſt was perceptible under the armpits, or in the thigh, attended with great itching; and in a few hours grew to a a very large tumour, joined with a burning heat of the part affected: Theſe ſymptoms were attended with a very acute fever, intire loſs of ſtrength, violent pains in the head, and redneſs of the eyes. An old country practitioner, famous in theſe parts for his judgment, cured perſons labouring under this ſevere diſeaſe in a ſhort time. He uſed firſt to the carbuncle the powder of an herb *, of which is given a complete hiſtory and figure in this work, made into a thin pultice with dregs § of beer: This pultice, gently warmed, was applied to the part affected, and the patient confined to his bed, who was at liberty to take whatever nouriſhment he liked, except milk, brandy, or the fleſh of pikes. During this time the patient drank plentifully of a decoction of this herb, collected during the time of its flowering; though the powder, applied as above, was prepared from the leaves, before the flower-ſtalk was produced. The carbuncle, from this treatment, did generally break in four-and-twenty hours, and the ſymptoms greatly abate. The wound was ſprinkled with ſal ammoniac, and healed in a ſhort time. This diſeaſe affected the cattle in different manners; ſome ſuddenly ſet a running

* Centaurea ſquamis ovatis, foliis pinnatis, foliolis decurrentibus, linearibus, ſerratis et integris. *Flor. Sibir.* Tom. II. p. 89. Tab. XLI.

Cyanus floridus odoratus Turcicus, ſeu orientalis major, flore luteo. *Hort. Lugd. Bat.* p. 211.

§ *Fæce cærevifiæ*; though I am inclined to think yeaſt is intended, which is uſually written *flos cerevifiæ*, or *fermentum cerevifiæ*.

running with all their ſwiftneſs poſſible, and continued ſo, till they dropp'd down dead: in others, carbuncles aroſe, which were dreſſed by the practitioner before-mentioned with the pultice juſt now deſcribed, mixing at the ſame time a large quantity of the herb with their food; and by this method great numbers were cured. A plant ſo well recommended, and which will grow in our own country, deſerves to be better known to us.

Thus much may ſuffice to lay before you ſome idea of the merit of this work; throughout the whole of which the author has ſhewn a complete knowlege of the botanic ſcience, among the firſt profeſſors of which he is deſervedly placed. He has given us the deſcriptions and figures of a great number of plants hitherto not deſcribed; and it is to be hoped, that he will continue his diligence in publiſhing the remaining twelve claſſes. But he ſhould not ſtop here; it were to be wiſhed, as ſo many ſkilful perſons were engaged ſo long in this dangerous and expenſive expedition, that their obſervations upon the remaining two kingdoms of nature ſhould be communicated to the public; as well as a complete hiſtory of their travels into theſe unknown parts of the world.

XXII. *A Letter to Mr.* William Watſon, *F. R. S. from Mr.* Philip Miller, *F. R. S. concerning a Miſtake of Profeſſor* Gmelin, *concerning the* Sphondylium vulgare hirſutum *of* Caſpar Bauhin.

Read May 3, 1753.

IN the abſtract of the *Flora Sibirica*, which you laid before the Royal Society at their laſt meeting, there was mentioned, that the inhabitants of Siberia eat the ſtalks of the *Sphondylium hirſutum C. B. P.* But I have great reaſon to believe Profeſſor Gmelin has miſtaken the ſpecies: For he deſcribes that plant as growing upward of ſix feet high; whereas the common ſort (as you well obſerved) ſeldom riſes much above half that height. Therefore I am of the opinion, that the plant mentioned by Profeſſor Gmelin is that ſpecies, which Doctor Breynius mentions, in his ſecond *Prodromus*, under the title of *Sphondylium maximum Tranſilvanicum Ricini folio*; the ſeeds of which I brought from Dr. Boerhaave's garden in the year 1727, where it was growing by the common ſort of Caſpar Bauhin, and in the ſame ſoil and ſituation was more than twice the height: and the ſame has continued in the growth of both theſe plants ſince, in the Chelſea-Garden; where the large ſort conſtantly riſes to a ſtem, at leaſt a month ſooner in the ſpring than the common ſort, and the leaves are much larger, leſs divided, and not ſo hairy; ſo that there can be no doubt of their being diſtinct ſpecies. The leaves of both ſorts I have ſent for your inſpection.

The ſeeds of that ſpecies of Doctor Breynius I have received from Siberia, by the title of *Sphondylium vulgare*, and Doctor Boerhaave told me, he had received the ſeeds from Auſtria, Hungary, and Peterſburgh, by the ſame name; ſo that it is certainly the common ſort in thoſe countries. And it is very uſual to find many miſtakes in the writers on botany; which has happened from their ſuppoſing, that the plants, which have been mentioned as common in one country, were the ſame with thoſe of the country where they inhabited. An inſtance of this you well remember of the *Parietaria minor ocymi folio. C. B.* which is the only ſpecies found wild in England; and ſo was by all the Engliſh botaniſts taken for the *Parietaria officinarum & Dioſcoridis C. B.* which are diſtinct ſpecies.

Many other inſtances might be mentioned of miſtakes, which have ariſen from the ſame ſuppoſition, were it neceſſary: but as you are ſo well acquainted with theſe things, ſo I ſhall not trouble you with the mention of any more at preſent, but remain,

SIR,

Chelſea, May 2, 1753.

Your moſt obedient humble ſervant,

Philip Miller.

XXIII.

XXIII. *A Letter from the Rev. Mr.* George Coſtard *to Dr.* Bevis, *concerning an* Eclipſe *mention'd by* Xenophon.

Dear Sir,

Read April 12, 1753.

OF what uſe the doctrine of eclipſes is in hiſtory and chronology, you know too well for me to tell you. The earlieſt account of any in the Greek hiſtory is that ſaid to have been foretold by Thales to the Ionians. Of this I ſhall ſay nothing more at preſent, for fear of repeating what I have already obſerved to you upon that ſubject. The next, that is generally taken notice of by writers, is, that in the firſt year of the Peloponneſian war mentioned by Thucydides. But there is another before that, as I apprehend, equally remarkable, which, as Mr. Jackſon (Vol. I. p. 426.) hath quoted me for, may deſerve ſome farther conſideration.

It is well known, that Herodotus, and other writers make Cyrus to have depoſed Aſtyages. On the contrary, Xenophon ſays, that Aſtyages was ſucceeded by his ſon Cyaxares, who left the kingdom to Cyrus by will. The truth, I believe, is, that Cyrus did not depoſe Aſtyages, and therefore ſo far Xenophon is right, but depoſed Cyaxares; in which he was deſignedly wrong. That he knew the Perſians forced the empire from the Medes, I think, appears from ſome no very obſcure hints even in the *Cyropædia* itſelf. But that argument I ſhall not enter into farther at preſent. In his *Anabaſis*, which is nothing

but a journal of his march, and where he relates things as he found them, he expreſly aſſerts it. For he ſays, that the Greeks in their return home, came down to the river Tigris, where had formerly been a large but at that time deſerted city called Lariſſa, inhabited formerly by the Medes. Ταύτην, continues he, βασιλεὺς ὁ Περσῶν, ὅτε παρὰ Μήδων ΕΛΑΜΒΑΝΟΝ τὴν ἀρχὴν, πολιορκῶν, οὐδενὶ τρόπῳ ἐδύναντο ἑλεῖν. ΗΛΙΟΝ δὲ ΝΕΦΕΛΗ προκαλύψασα ἠφάνισε, μέχρις οἱ ἄνθρωποι ἐξέλιπον, καὶ οὕτως ἑάλω.

Tho' Xenophon calls this a cloud, yet the word ענן *anan*, probably made uſe of by thoſe, from whom he received his account, not only ſignifies a cloud, but any obſtacle in general. That ſuch effects could be owing to no common cloud, I imagined muſt be evident enough. And as the year before Chriſt 547, from a number of arguments, too long to be here inſiſted on, appeared to me to be the year, when Cyrus finiſhed his reduction of the Median empire, I was naturally led to try, whether there was any ſolar eclipſe that year, that could be the cauſe of ſo remarkable a darkneſs.

The geography of the eaſt is ſo very imperfect, that it may be difficult to determine the ſituation of this Lariſſa. For Xenophon hath given no other account of it, than that it lay on the banks of the Tigris. It is not improbable however, as Bochart thinks, that Xenophon inquired upon the ſpot, What ruins thoſe were? And was anſwered לרסן *Lareſen*, i. e. *Reſen's*, or the ruins of *Reſen*. But this he eaſily miſtook, or changed into *Lariſſa*, a name he was much better acquainted with.

Reſen

Resen is a place mention'd *Gen.* x. 12. and there said to lie between Nineveh and Calah. This perhaps may help us in some sort towards its situation. *Mausil* (says Abulfeda) *metropolis est regionum Mesopotamiæ. Imposita est Tigri, a latere ejus occiduo. Ex adversum, a latere ejus eoo, est civitas Nineveh ruinis sepulta. Ad austrum Mausilæ effundit sese Zabus minor in Tigrim.*

In Abulfeda, the longitude of Mausil is 67°, and its latitude north 36° 30′. But in Ulugh Beigh, and Nassir Ettusi, it hath longitude 77°. The last of these two authors agrees with Abulfeda, as to its latitude, but the former of them assigns it 34° 30′; which makes a difference of two degrees, or 120 geographical miles. Chrysococca, in his tables, hath ΜΟΥΣΟΥΛΙΝ, to which he assigns longitude (ξθ) 69, and latitude (λε) 35. This author reckons his longitudes ἀπὸ τῆς ἄκρας δυτικῆς θαλάττης, as Abulfeda doth his. But Nassir Ettusi and Ulugh Beigh begin theirs از جزاير خالدات from the Fortunate Islands. The difference between the two computations is 10 degrees, according to Abulfeda, tho' others make it 17° 30′. Varenius, and modern geographers, place Mousol in longitude east from London 43°, and in latitude north 34° 32′: And this, I suppose, is as exact as any.

In the year before Christ, therefore, 547, the apparent time of the true conjunction was at Greenwich, October 21d 23h 39′ 18″.

	s	°	′	″
The place of the luminaries	6	24	9	0
Moon's latitude north			46	24
Semidiameter of the earth's disk			61	18

Semidiameter

	′	″
Semidiameter of the penumbra	33	11

Beginning of the central eclipſe, 22ʰ 39′ 7″.
End of the central eclipſe 22ᵈ 0ʰ 55′ 55″.
Duration 2ʰ 16′ 48″.
Angle of the moon's way with a circle of latiude, 84° 25′.

Moon's ſemidiameter	16′ 50″.
Sun's ſemidiameter	16′ 21″.

Angle of the moon's way with a circle of latitude, 84° 25′.

	°	′	″
The ſun roſe centrally eclipſed, in longitude weſt from Greenwich,	54	46	10
In latitude north	71	44	40
The ſun ſet centrally eclipſed, in longitude eaſt from Greenwich,	91	6	50
In latitude north,	23	9	40

The place of the center was at the following times, as computed at Greenwich, thus,

Times.					Longitudes.			Latitudes.		
	d	h	′	″	°	′	″	°	′	″
October	21	23	39	47	23	7	30E	39	48	40N
		23	47	31	28	36	40	34	20	50
	22	0	2	31	37	13	30	30	24	30
		0	17	31	57	19	40	26	56	40

' By this table it is eaſy to ſee, that the center of the ſhadow croſſed the deſerts of Arabia, and then paſſed over the Perſian Gulph below the mouth of the Euphrates. But if you will make the ſame allowance, as I did in my laſt, for the moon's acceleration, or

or the ſmall retardation of the earth's diurnal motion, the place of the center will be found at the following times, as reckoned at Greenwich, thus,

Times.					Longitudes.			Latitudes.		
	d	h	′	″	°	′	″	°	′	″
October	21	23	2	31	39	1	40E	34	20	50N
		23	17	31	47	38	30	30	24	30
		23	32	31	68	9	40	26	56	40

By this table then it appears, that the center of the ſhadow paſſed more northward than the former track, and went over Kerkiſia, not improbably, the Carchemiſh of the prophet Jeremiah, and a ſmall matter to the north of Bagdad. It is not improbable therefore, that it croſſed the Tigris not far from the place where, it ſeems by Xenophon's account, Lariſſa was ſituated, and where, conſequently, it would cauſe ſuch a darkneſs, as might well be attended with the effects he mentions.

This eclipſe is, I take it, no inconſiderable acquiſition to hiſtory and chronology, and is at the ſame time a confirmation of the ſuſpicion, that, in theſe very antient ones, there is ſome allowance or other to be made for the influence of ſome cauſe, whatever it may be, hitherto not fully determined. This muſt be left for future obſervations. In the mean time however it may be of ſervice to the ſcience of aſtronomy, to examine all the paſt eclipſes, that can be come at, and compare them with circumſtances in the beſt manner we are able.

I have three more by me, which may ſome time or other make their appearance in the ſame manner

with the preſent one. In the mean time I ſhall add, that in Ebn Younis's eclipſe, in the year after Chriſt 979, the time of the mean oppoſition was, by my calculation, May $14^d\ 3^h\ 50'\ 30''$.

	s	°	′	″
The ſun's place	1	27	53	0
The moon's place in the ecliptic	7	27	53	0
Moon's latitude north			37	42
Place of her apogee	4	13	50	46
Place of her node	2	5	4	14
Sun's mean anomaly	11	1	29	47
Place of his apogee	2	25	29	30

Digits eclipſed $8^d\ 4'$.

Theſe numbers are ſomething different from thoſe, which, I think, I gave Dr. Bradley ſome time ago. For I had made, ſome how or other, a ſmall miſtake in the place of the ſun's apogee, which was kindly hinted to me by Mr. M. I have, ſince that, gone over the whole calculation again, and believe it is pretty exact, and agrees well enough with what Ebn Younis relates, that *Spatium, quod eclipſatum fuit de diametro ejus* (lunæ) *fuit amplius quam octo digiti, et minus quam novem.* And afterwards, *Et erat luna, in hac eclipſi, in propinquo diſtantiæ ſuæ mediæ.*

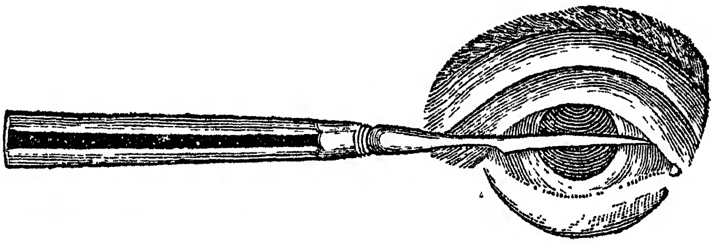
I have been led inſenſibly farther than I intended at firſt, which was only to explain the grounds of what I had ſaid to Mr. Jackſon. But your love to the ſcience will excuſe the trouble I have given you, and make any farther apology needleſs. I am, dear Sir,

Oct. 3, 1752. Your very affectionate, *&c.*

G. Coſtard.

XXIV.

XXIV. *A Deſcription of a new Method of opening the* Cornea, *in order to extract the* cryſtalline Humour; *by Mr.* Samuel Sharp, *Surgeon to* Guy's Hoſpital, *and F. R. S.*



Read April 12, 1753.

THE operation of diſcharging the cryſtalline humour from the eye, for the cure of that ſpecies of blindneſs call'd a cataract, was a few years ſince invented by Monſ. Daviel, who has perform'd it on great numbers of patients, and continues ſtill to practiſe it with remarkable ſucceſs, as I have lately learned from unqueſtionable authority *. Suppoſing it therefore admitted, that the extraction of the cryſtalline humour has been found by experience to be a uſeful method of cure, I here take the liberty of laying before the Society a new manner of making the inciſion of the cornea, by by which, I flatter myſelf, Monſ. Daviel's operation will be very much ſhorten'd, the patient will ſuffer leſs pain, and every ſkilful operator will be equal to the undertaking.

Place

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* Mr. Morand, perpetual Secretary of the Academy of Surgery at Paris.

Place the patient in the ſame ſituation as for couching, either opening the eyelids with your fore-finger and thumb, or letting an aſſiſtant raiſe the upper eye-lid, whilſt you yourſelf keep down the under eye-lid. Then, with a ſmall knife, the figure of which is here repreſented, holding its edge downwards, make a puncture through the cornea near its circumference into the anterior chamber of the eye, in ſuch a direction, as to carry it horizontally, and oppoſite to the tranſverſe diameter of the pupil: after which you are to paſs it towards the noſe, through the cornea from within outwards, as near to its circumference, as in the firſt puncture.

When you have made the ſecond puncture, puſh the extremity of the blade one ſeventh of an inch beyond the ſurface of the cornea, and immediately cut the cornea downwards, drawing the knife. towards you as you make the inciſion. After this, you preſs gently with your thumb againſt the inferior part of the globe of the eye, in order to expel the cataract, and the operation finiſhes, according to the different circumſtances, as in the manner propoſed by Monſ. Daviel.

One extraordinary benefit ſeems to ariſe from the uſe of this ſingle inſtrument, and perhaps from the ſhape of its blade, which increaſes in breadth all the way towards the handle: for, by this means, the punctures are ſo exactly fill'd up by the blade, that very little of the aqueous humour is diſcharged before you begin to make the inciſion, and conſequently during this time, the cornea preſerves its convexity; whereas by uſing one inſtrument to puncture, and others to dilate, the cornea immediately becomes

becomes flaccid, upon the issue of the aqueous humour, and renders the operation tedious and embarrassing, as I myself have found by experience in one patient, on whom I performed the incision of the cornea with a pair of scissars, as recommended by Mons. Daviel.

XXV. *Experiments by* Francis Hume, *M. D.* *on* Fish *and* Flesh *preserved in* Lime-water, *communicated by* John Clephane, *M. D. F. R. S.*

Read May 3, 1753.

WITH a design to find out how long I could keep fish and flesh fit to eat in lime-water, I put two haddocks, and a pound of beef, in different pots full of lime-water, and corked them well. They stood in our cellar 18 days.

I then took out one of the fish: it was sweet, sound, and firm; I boiled one part of it, and I broiled the other: it eat well, and had not the least taste of lime-water; but was not just so firm as a fresh fish. But when I open'd the beef-pot, to my great surprize, it stunk abominably.

I poured the lime-water from both pots, and put in fresh lime-water. This stood 4 weeks longer; the remaining fish was quite fresh, and a little swelled, but, when I boil'd it, dissolved to a jelly. The flesh was very putrid.

Thus lime-water appears to preserve fish, but not flesh.

Dr. Alſton's experiment was made with fiſh, and Dr. Pringle's with fleſh; which has made the former ſay, that lime-water withſtood corruption ſtrongly; and the latter, that it did it but weakly, if at all.

Edinburgh, April 6, 1753.

I LATELY repeated the experiment more fully, and with the ſame ſucceſs. On the 26 of March I put a haddock into a pot of common water. I did the ſame to a piece of beef: the water was changed every day. At the ſame time I put a haddock into a pot of lime-water, and did the ſame with a piece of beef: at the ſame time I hung a fiſh and a bit of fleſh in the air. On the ſecond of April the fiſh and fleſh in the air were a little corrupted and dried; the fleſh and fiſh in common water ſmelt ſtrong; the fiſh in the lime-water was ſweet, and the lime-water good, and are ſo at preſent; but the fleſh ſmelt rather worſe than that in common water changed every day, and the corruption had quite overpower'd the ſmell of the lime-water.

All this you have my leave to ſhew, as a confirmation of the former experiment.

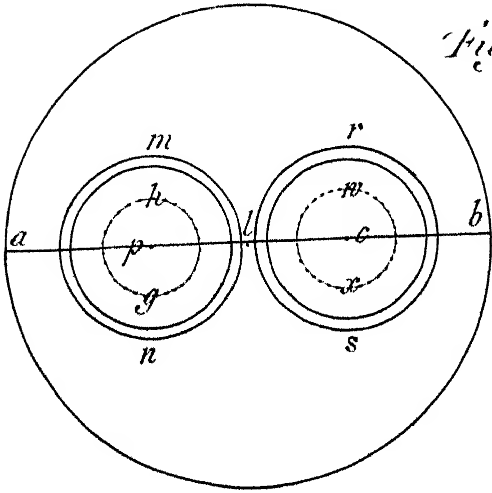


Fig. 5.

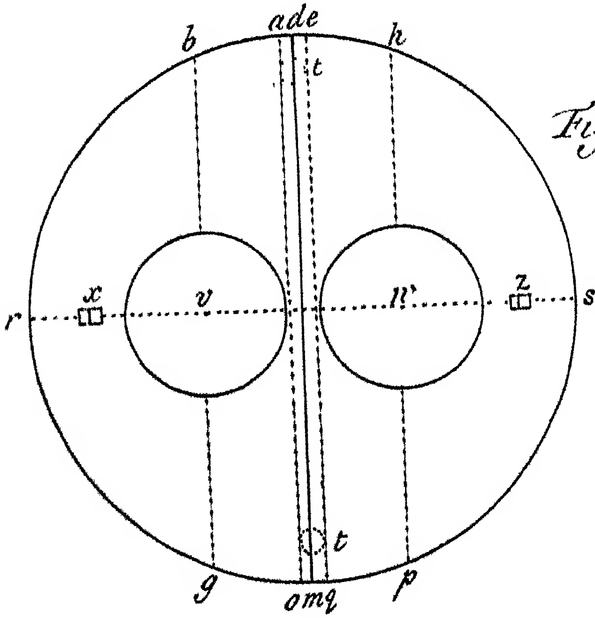


Fig. 7.

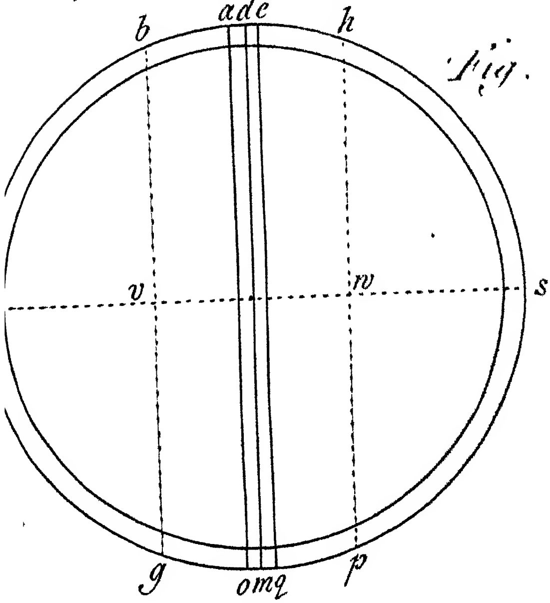
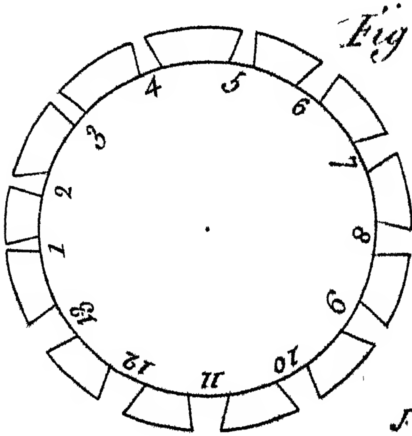


Fig. 6.

Fig. 8.



J. Mynde sc.

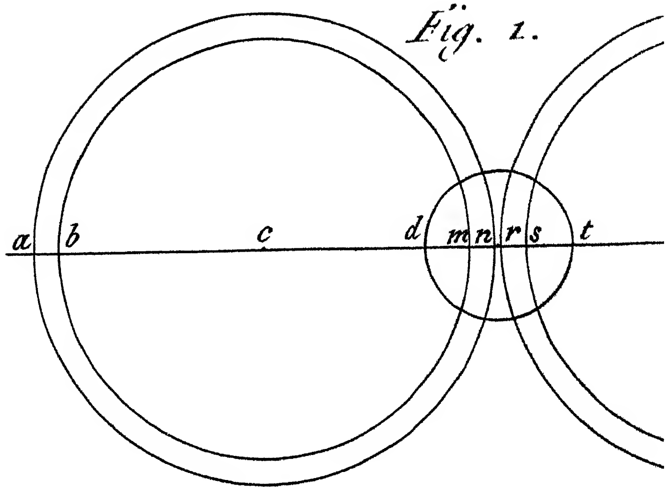


Fig. 1.

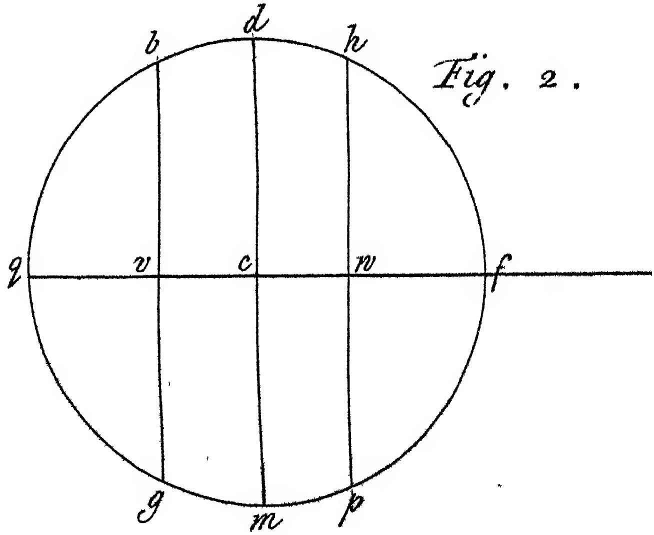


Fig. 2.

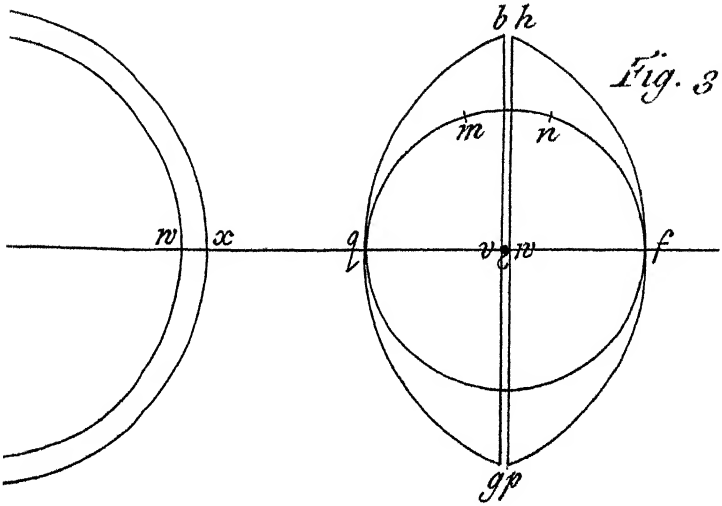


Fig. 3.

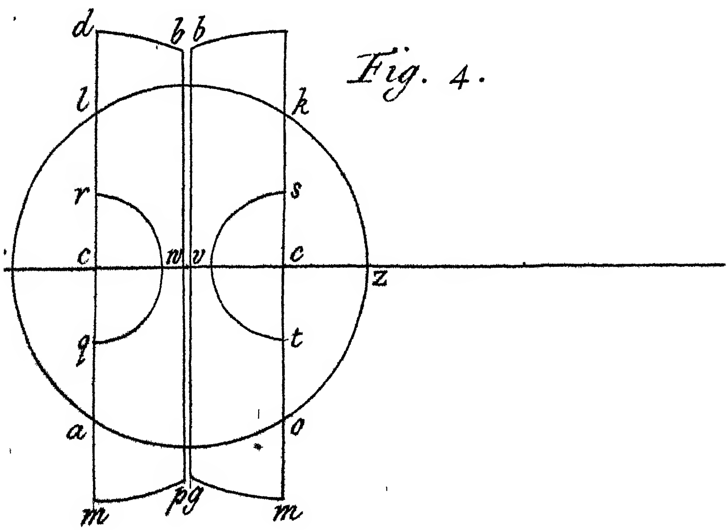


Fig. 4.

J. Mynde sc.

XXVI. *A Letter from Mr.* James Short, *F. R. S. to the right honourable the Earl of* Macclesfield, *President, concerning a Paper of the late* Servington Savery, *Esq; relating to his Invention of a new* Micrometer.

My Lord,

Read May 10, 1753.

IT is now above a year ſince I received a letter from the Rev. F. Pezenas, profeſſor of hydrography to the French King at Marſeilles, in which he informed me, that M. Bouguer had read, before the Royal Academy of Sciences at Paris, in the year 1748, a memoir, in which he deſcribes an heliometer; which is an inſtrument, conſiſting of two objective glaſſes, for meaſuring the diameters of the planets. He ſaid alſo, that this memoir was actually in the hands of M. de Fouchy, Perpetual Secretary of the Academy, or at the Royal Printing-houſe; and that it was regiſter'd in the minutes of the Academy for the year 1748.

Immediately after reading this letter, I recollected to have heard a paper upon the ſame ſubject from the late Servington Savery, of Exeter, Eſq; read before the Royal Society, about the year 1743. I therefore had recourſe to the minute-book of the Society for that year, where I found the following minute, which I copied in the preſence of the right honourable the Lord Charles Cavendiſh, then Vice-Preſident:

"A

I have therefore contrived ſome dioptric teleſcopes, and a reflecting one; either of which (by repreſenting the object double) will, if well made, anſwer the deſign.

Fig. 1.

Repreſents the whole body of the ſun, as it appears double, and magnified in the teleſcope. Let *an* be the diameter of the one, and *rx* of the other image of the ſun *in perigæo*; ſo ſhall *nr* be the diſtance between the two images at that time; which meaſured with the micrometer is equal to (ſuppoſe) 10 ſeconds.

Let *bm* be the diameter of the one ſolar image, and *sw* of the other, when *in apogæo*: ſo ſhall *ms* be the then diſtance of the ſolar images, meaſuring with the micrometer (ſuppoſe) 1 minute 10 ſeconds. The difference of theſe two obſervations, 1 minute, is the apparent diminution of the ſun's diameter.

The little circle, whoſe diameter is *dt*, is the whole area viſible at once in the teleſcope, which is not one third part of the magnified diameter of the ſun: but ſince both *nr* at one time, and *ms* at another time, are viſible within the teleſcope's area, (if good inſtruments are procured) I can ſee no difficulty in performing what I have propoſed above more accurately than it has ever yet been done, except this one (which ſome time ſince Mr. Graham in a letter to me mention'd) *viz.* that of defining the ſun's diſk truly: and I think to do that to good perfection, is beyond human art. A teleſcope for this uſe may be made to magnify the ſun's diameter to any degree whatſoever, not exceeding ſuch degree, as will make any part of the line *ms* fall without the area of the teleſcope: and I think it will be very difficult to make one with

a charge

a charge ſo great, as not to have more than a geometrical minute of the ſun's apparent diameter viſible at once.

Since the ſun is an object ſo very remote, the pencil of rays flowing from the center of its diſk, and incident all over an object-lens (tho' it ſhould be a foot broad) would not differ ſenſibly from a perfect cylinder within the diſtance of above 100 miles from its baſis at the lens; tho' in reality the whole pencil is an acute cone, whoſe angle at the vertex is almoſt evaneſcent.

Hence it follows:

That if the two poles of two equal object-glaſſes are placed at the diſtance (ſuppoſe) of a foot from one another, the two centers, *c*, *v*, of the two ſolar images muſt, as to ſenſe, remain always at that very ſame diſtance (*viz.* 1 foot) from one another, tho' the ſun ſhould be placed ten times as far off as it now is: but ſince the ſun's greater diſtance would diminiſh the diameters of both of the ſolar images; *m n*, added to *r s*, muſt be the true difference of the apparent diameters of the images (and alſo of the ſun) at different times.

According to Mr. Azout (*Harris's Lexic. Techn. Vol.* I. *ſee* SUN), the apparent diameter of the ſun never exceeds 32′ 45″; whence its radius never exceeds 16′ 22″ 30‴; the tangent whereof is about 476,328 (if I miſtake not) to the radius 100,000,000.

As the ſaid tangent : to the ſaid radius : : ſo half an inch : to 104.96 inches, and decimal parts.

According to this,

If the focal length of a lens is 104.96 inches and parts, it cannot collect the ſun's rays to a leſs focus

at the time of his perigee than one inch in diameter, or half an inch radius.

Fig. 2.

The whole circle repreſents a well-center'd object-lens, whoſe focal length is (as above calculated) 104.96 inches and parts (rather a little leſs, that the two images may be ſure not to touch one another.) Let the two diameters *d m*, *q f*, divide it into four quadrants, but the diameter *q f* muſt be occult, or delible. Let *c w* be half an inch, and *c v* equal to it. Through *v* (and alſo thro' *w*) let a chord-line be drawn parallel to the diameter *d m*, viz. *b g*, *b p*. Thro' the ſaid chord-lines *b g*, and *b p*, and alſo thro' the diameter *d m*, divide the lens into four parts.

Fig. 3.

Let the ſtrait edge of the fruſtum *b v g q* in the preceding figure be cemented faſt to that of the ſimilar fruſtum *b w p f* of the ſame lens, as they appear in this preſent Fig. 3. wherein, for the eaſier underſtanding the placing them, I have noted each fruſtum with the ſame letters it was noted withal in the preceding Fig. 2. Having then with barm faſtened a white paper all over both ſides of the lens I made for trial (which I did, not only to ſecure the cemented joint from breaking, but to prevent the injury which the poliſh might receive in cutting and grinding the edges) I deſcribed a circle *q m n f* on the center *c*, fit for the tube I had to put in it; and having made it round, and waſh'd it clean, after the edges were ground true, that nothing ſandy might hurt the poliſh, I ſoak'd it in clean water, till I could eaſily take off the

the paper. I alſo, before I took off the paper, mark'd one end of each fiuſtum in the edge at *m* and *n*, that if they fell aſunder, they might be cemented together again the ſame way. This model (made of a ſpectacle-glaſs about 12 or 13 inches focus) gave me encouragement to try the following one, which I thought better.

Fig. 4.

I made my ſecond model of the two middle fruſtums *m c d h w p*, *m c d b v g*, of the lens Fig. 2. by cementing their edges, *h w p*, *b v g*, together, as they are placed in the preſent Fig. 4 ſo the pole *c* of each part muſt conſequently be half an inch (ſuppoſing its focal length is about 104 inches) from the middle where *c* ſtood in Fig. 2. *viz.* the pole of one fruſtum where *v*, and of the other where *w* now ſtands. I left open at each pole a ſemicircular aperture *r w q*, *s v t*, about two thirds of an inch diameter, and cover'd all the reſt of the circle *a x l k z o*, to which I had cut it fit for the tube. The focus of the lens I made it of was about 3 feet.

Note, The rays of red light in the two ſolar images will be next to one another in both theſe models, which, I take it, will render the ſun's diſk more eaſy to be obſeıved than the violet ones. This I mention, becauſe the glaſſes in theſe two ſorts are ſomewhat priſmatical, but moſtly thoſe of my firſt model, which could therefore bear no great charge. Alſo the fruſtum on the right-hand of my firſt model renders the ſolar image at the focus on the left, and that on the left-hand renders it on the right: but it is not ſo with the ſecond model, or with the next contrivance, which is the beſt, if well made.

Fig. 5.

In this the greateſt difficulty conſiſts in getting two well-center'd object-glaſſes, whoſe focal lengths are equal; for it is neceſſary they ſhould be ſo, becauſe they are to be combined with the ſame convex eye-lens (common to them) at the ſame diſtance. *a b* is the diameter of a plain braſs plate, which may be two inches and a half broad, or ſomewhat leſs; two ſhort equal cylindric braſs tubes, *m n*, *r s*, muſt be faſtened thereon, with their centers *p c*, equidiſtant from the center *l* of the plate, and diſtant one inch from one another in the diameter *a b*, as the figure ſheweth. In the tubes muſt be put two equal object-glaſſes of the focal length of $104\frac{9}{10}$ inches, or rather ſomewhat leſs, as aforeſaid. Through the plate there muſt be made in the middle of each tube a round aperture, *viz.* *b g*, *w x*, whoſe diameters muſt be proportion'd to the focal length of the eye-lens, and not exceed the third part thereof, leſt the object appear confuſed.

And ſince it is ſcarce poſſible to center an object-lens to very good perfection, thoſe in the two cylinders (tho' put in by a good artiſt) may happen to render the two ſolar images at too great a diſtance from, or too near to one another. But this fault, if not too great, may be remedied, by turning one or both of the lens's a little way round; and then their eccentric poles will by that means be brought nearer to, or further from, one another; and when they are once well plac'd, there ſhould be a mark made in each lens, and its cylinder; that if it is taken out to be wiped, it may be put in again the ſame way. There ſhould alſo be a different mark in one of

the glaſſes, that each may know its own cylinder. They muſt both of them be very cloſe all round to their reſpective cylinders; otherwiſe one lens may ſlide nearer to or further from the other; which if it ſhould in the leaſt degree, between the firſt and ſecond obſervation, all the labour would be loſt.

Either of theſe three parts of double lens's may be combined with a convex eye-lens as uſual, and have a micrometer placed at the common focus.

Such a double lens, of either ſort, may be proved whether it is well compoſed or not, without the trouble of combining it with its eye-lens; by holding it in the ſun's rays, as one would a burning glaſs, and applying a piece of white paper at its focus, where, I apprehend, the two ſolar images will appear as diſtinct as when an eye-lens is applied, tho' not ſo large; and each of them one inch broad, if the focal length is as above, *i. e.* almoſt $104\frac{9}{10}$ inches.

After the ſame manner may the double object-mirror of a reflecting teleſcope for this uſe be proved.

Fig. 6.

The circle *bdhpmg* is the circumference of a concave mirror made of black glaſs: it muſt be very thick, that it may not ſpring or bend with any thing that preſſeth on it to keep it faſt; for that may injure the concavity of it.

The circle within it on the ſame center *c* ſheweth, that the concavity thereof muſt not be continued quite home to the very edge of the mirror, but the little ſpace between the two circles muſt be ground very true on a plain. The prick'd lines muſt not be drawn; they are only to indicate where the poles

v w of the two fruſtums muſt be brought, after the mirror is diametrically biſected.

Let the concave ſide be defended, by paſting a paper all over it, and then let it be divided with a ſaw in the diameter *d c m*; taking care that the ſaid diameter be in the middle of the kerf, which may be as broad as the ſpace between the lines *a o*, *e q*. Let the aſperities of the edges of both fruſtums be ground off, that they may be very ſtrait after their being ſawn.

Fig. 7.

Repreſents a thick round plate of braſs very plain, and equally thick all over, having lines drawn on it, as on Fig. 2. alſo one line on each ſide of the diameter *dm*, equidiſtant from it, and parallel thereto. The diſtance of theſe two lines *a o*, *e q*, from one another equal to the kerf of the ſaw, which divided the mirror. The diameter of this plate muſt be equal to that of the mirror before it was divided.

On the under ſide of the plate muſt be two pins faſtened thereto, *t t*, their diameters equal to the kerf of the ſaw, that they may keep the two fruſtums of the mirror at the ſame diſtance from each other that they were before their diviſion; ſo ſhall their circular edges be extended as far as the circumference of the plate, and their ſtrait edges touch the ſaid pins in the lines *a o*, *e q*.

The end of the tube muſt be turn'd on the inſide exactly to fit the plate and mirror, that they may not ſlide any way, for that would ſpoil the obſervations.

In the diameter of the plate *r s*, on the points *v*, *w*, diſtant half an inch from *c*, the center of the plate, and

and a whole inch from each other, let a circle for the aperture of each fruſtum, of a prop·r ſize, according to the intended charge of the teleſcope, be deſcribed, and cut out. Alſo in the ſaid diameter, equidiſtant from the center *c*, *viz.* at *x* and *z*, let there be a ſcrew for each fruſtum, to elevate it a little from the plate, as ſhall be needful.

Let there be a ſpring contrived to preſs on the backſide of the one fruſtum *o r a* againſt the point *v*, being the middle between the edge *a o*, and the ſcrew *x*, to keep the fruſtum cloſe to the plate at the points *a o*, and alſo cloſe to the ſcrew *x*, when it is ſcrew'd in. Let the like be alſo done on the back of the other fruſtum *e s q*.

I ſay then,

1. That before the two ſcrews are put in at *x*, *z*, the two fruſtums of the mirror will lie plain on the plate of braſs, and have one pole at *c* common to them, and, conſequently, will collect all rays, which, during their incidence, are parallel to the axis of the tube to one common focus in the ſaid axis of the tube, juſt as they would have done before the mirror was divided.

2. But when the two ſcrews *x z* are put in their places, and ſcrew'd a little way through the braſs plate, they will lift the two fruſtums free from the plate at their circular edges, *viz.* at *r* and *s*, while their ſtrait edges, *a o*, *e q*, are kept to touch the plate with both their ends (not in the middle, by reaſon of the mirror's concavity) by the preſſure of the ſprings, as was mention'd above. By this means the pole *c* of the fruſtum *o r a* will be removed from *c* toward

toward *r*; and likewiſe the pole *c* of the other fruſtum *e s q* be removed from *c* towards *s*, more or leſs according to the quantity of the elevation of each fruſtum by the ſcrew that raiſeth it: ſo that now there will appear at the focus two ſolar images; whereas there was but one, before the ſcrews were put in.

By moving the ſcrews, the two ſolar images may be brought to any diſtance from one another; but care muſt be taken not to raiſe one fruſtum more than the other, and the two ſolar images muſt almoſt touch one another at the time of the perigee; otherwiſe it muſt be better adjuſted.

This teleſcope may be finiſhed with a ſmall elliptical ſpecillum of black glaſs, ground plain on its reflecting ſurface, and a convex eye-lens, like that deſcribed by J. Hadley, Eſq; F. R. S. in *Phil. Tranſ.* N° 376. A micrometer may be contrived for it at the common focus, near the eye-lens.

Such a double object-ſpeculum would be capable of a vaſt improvement, by combining it with a concave ſpecillum, which would reflect the images thro' a hole in the center *c* of the ſaid ſpeculum to fall on a convex eye-lens, after the manner of our new ſort of reflecting teleſcopes, was it not for the difficulty of adapting ſuch a micrometer to it as would exactly meaſure minutes and ſeconds; for the eye-glaſſes of ſuch having uſually a pretty large focal length, would bear much larger diviſions on a micrometer, than Mr. Hadley's with a ſmall eye-glaſs can do, tho' their charges ſhould be equal, or that of the former did exceed.

I find that large object-glaſſes for teleſcopes are not commonly well center'd, with their poles in the very middle

middle of them. I ſent for two to London, both of which were faulty; I therefore return'd them, and had two ſent me again, as eccentric well nigh as the former ones.

Harris's *Lexicon Techn.* Vol. I. (ſee *Optics*) gives a rule for centering optic-glaſſes; but I think the following may be more ſure and handy for a glaſs-grinder's uſe, and ſoon try whether a convex lens is well center'd.

Fig. 8.

Repreſents a round plate of braſs, conveniently thick, and well harden'd by hammering (were it not for the ruſt, harden'd ſteel would be better), having many notches round it, one a little wider than that which is next to it, and number'd 1, 2, 3, *&c.* in their proper order, each of them wider at the bottom than at the entrance. I fitted ſuch a notch to the thickeſt ſide of one of the glaſſes I had from London, ſo as the edge enter'd it but a little way, not half the depth thereof; but, on trying the oppoſite ſide, it went in, the whole depth thereof, and would have gone deeper, if the notch had been ſo cut: I then ground the lens narrower on that ſide which was thinneſt, until I found it was at that place as thick as where I firſt try'd it in the notch. After this manner I reduc'd the glaſs to an equal thickneſs on its four quarters, and then ground off from other places what was needful to bring it circular. I alſo took care, when I tried it in the notch, that the lens ſhould not be warmer on the one ſide than on the other by grinding, but tarried till I thought it thoroughly cold; and was alſo careful not to thruſt it in harder on the one ſide than on the

opposite side; for I could plainly observe a difference afterward, if I neglected to mind both these circumstances, or indeed either of them *.

XXVII. *A Description of a Contrivance for measuring small* Angles, *by Mr.* John Dollond; *communicated by Mr.* J. Short, *F. R. S.*

Read May 10, 1753.

LET an object-glass, of any convenient focal length (being truly ground and well center'd) be divided into two equal parts or segments, by cutting it strait through the center; and let a piece of machinery be so contriv'd, as to hold these two segments in the same position to each other, as they stood in before they were cut asunder; and to be capable at the same time of drawing them to different distances from that position, in the manner, as is represented in the figure.

Each of these segments will form a distinct image of any object, to which they are directed; differing in nothing from that, which might have been made by the whole glass before it was cut, except in brightness. And while these segments are held in their original position, the images will coincide, and become one single image as at first; but, in proportion as they

* Dr. Smith, in his Complete System of Optics, published in 1738, has described a very accurate and ready method of centering object-glasses, which was always used by the late Mr. George Graham, from whom the doctor had it.

they are drawn off from that ſituation, the images will ſeparate more or leſs, according to the diſtance they are drawn to. By this means the images of two different objects, or of different parts of the ſame object, not very far from each other, may be brought to a contact or coincidence at the focus: and this coincidence may be view'd to a very great nicety with a proper eye-glaſs.

The meaſure of the angle ſubtended by the two objects, whoſe images are thus brought to a coincidence, depends upon three things: Firſt, a careful obſervation of the coincidence of the images: Secondly, an exact meaſure of the diſtance, which the glaſſes are drawn out to from that ſituation, which makes the image ſingle: And, laſtly, a true knowlege of the focal diſtance of the glaſs. How the angle is to be found from theſe meaſures, and how it may likewiſe be come at, by viewing two land-objects at a convenient diſtance, will be ſhewn hereafter in the explanation of the figure. It is eaſy to underſtand, in the mean time, that the angle will be meaſur'd with more accuracy, in proportion to the length of the glaſs, which is uſed for that purpoſe; but the difficulty of managing long teleſcopes is no leſs apparent. Therefore the moſt practicable method of uſing this micrometer to advantage, is, to apply the divided object-glaſs to the object-end of a reflecting teleſcope: for, as the apertures of theſe ſort of teleſcopes are large in proportion to their lengths, they will admit of very long glaſſes; nor will the meaſures be any way affected by the metals or glaſſes, which the reflector is compoſed of: and the angles will be found in the ſame manner, as though the images were view'd with a

single eye-glass, in the manner of a common refracting astronomical telescope; but with this advantage, that, as the images will be exhibited larger and distincter by the reflecting telescope; and as every part thereof will be much more manageable than a long refracting telescope; so the contact or coincidence of the images will be more accurately observ'd.

It would be however unnecessary now, as well as improper, to say much about the advantages of this method above those, which have hitherto been put in practice; because, as a machine is now making for this purpose, the experiments, which will shortly be tried, will be more convincing, as well as more intelligible, than any thing that might be offer'd at present.

Explanation of the Figure.

The two semicircles represent the two segments of the object-glass, whose centers C and D are drawn off to the distance CD, and the points A and B are two objects, or different parts of the same object; therefore the lines ACG and BDG represent two rays that pass thro' the centers or poles of the segments, and are therefore not at all refracted, but go strait through to G, where they intersect; and G being the respective focus to the distance of the objects from the glass, the two images will coincide at that point. It appears from the figure, that $AB : CD :: GH : GE$; and from a common proportion in optics, $GH : GE :: HE : EF$. Therefore, $AB : CD :: HE : EF$; F being the focus of parallel rays; and consequently

consequently the angles AEB and CFD are equal. That is, the angle subtended by the distance of the centers of the segments from the distance of the focus of parallel rays is equal to the angle subtended by the distance between the objects A and B from the end of the telescope.

XXVIII. *A Letter to Sir* Peter Thompson, *Knt. F.R.S. containing Experiments on the* Copper Springs *in* Wicklow *in* Ireland, *and Observations thereon, by* John Bond, *M. D.*

SIR,

Read May 10, 1753.

YOU may remember I had the honour of spending an evening with you in June last, and happen'd to mention a spring in the county of Wicklow in Ireland, which was supposed to have the surprising quality of changing iron into copper. But your constant love of truth, and strong aversion to vulgar errors, made you doubt the fact, which, at that time, I could only affirm on the report, which prevailed among the curious in that country. You then proposed several judicious queries, and seemed desirous of being further informed concerning it, which raised my curiosity to take the first opportunity of inquiring more particularly into the foundation of that marvellous account.

Having soon afterwards occasion to go to Dublin, I went to the spring, which is from thence about thirty-eight miles, and made several experiments on the

the water; the result of which I beg leave to present you with, hoping it may afford you some satisfaction in explaining that process, of which you so justly doubted the account given by some credulous authors, who mistook it for a real transmutation; a ridiculous doctrine, which destroys the essential qualities of bodies, which were impressed by the Great Creator on all material substances, in order to distinguish them from each other, and therefore are intransmutable.

As the history of this discovery has already been accurately related, in several papers read before the Royal Society, of which you have the honour of being a worthy member, I shall endeavour to avoid repetition, and confine myself to the chemical analysis of the water, which, I am told, has been hitherto omitted.

This water flows from a rich copper mine, and is of a sharp acid taste, and light-blue colour. It is received and collected in pits, wherein iron bars are put, which, after lying in the water about three months, are intirely consumed, and at the bottom of the pits, a quantity of copper, greater than that of the iron is found, in the form of coarse sand. This fact is confirmed by profitable experiments, often repeated since the discovery, the honour of which is due to Mr. Matthew Johnston, a worthy old gentleman, and one of the proprietors of the mine, who first proposed this method of collecting the copper.

As this effect is certainly produced by some active principle in the water, I shall first inquire, what this principle is, and mention particularly the experiments, which I made with this view, that you may the better judge of the inferences deduced from them.

Experiment 1.

Into ſome of this water, taken out of the ſtream above the pits where the iron bars are placed, I pour'd a ſolution of an alkaline ſalt, which raiſed a ſtrong efferveſcence, and precipitated a large quantity of a dark-brown ſubſtance.

Corollary. This experiment ſhew'd, that the water contain'd a ſtrong acid, with a ſolution of the ſubſtance precipitated.

Experiment 2.

I put ſome aqua-fortis, or ſpirit of nitre, into ſome water taken out of the ſame place; and obſerved, that the ſtrong acid immediately deſtroy'd the blue colour.

Corollary. From this experiment we may conclude, that the ſubſtance, which was precipitated by the alkali in the firſt experiment, was ſo perfectly diſſolved by the acid ſpirit in the ſecond, as to tranſmit all the rays of light.

Experiment 3.

Some ſmall iron nails put into this water were in four minutes ſo cloſely cover'd with ſome ſubſtance of a copper colour, that, with a magnifier of half an inch focus, I could not diſcern the iron through it. In that time the nails gained four grains. The water had the ſame effect on ſilver and tin, but not on gold.

Corollary. The colour and increaſe of weight were owing to the adheſion of the particles of the matter diſſolved in the water by an acid, that could not penetrate gold.

Experiment

Experiment 4.

In order to determine the quantity and quality of the matter in the water, I put two drachms of ſmall iron nails into three ounces of it, and let them ſtand twenty-four hours; then examin'd, and found the ſurface of the water cover'd with a thick ſcum, like that of a chalybeat Spa. It loſt the blue colour, and ſharp vitriolic taſte. It was quite tranſparent, and at the bottom there was a quantity of a brown ponderous powder, which, when dried, weigh'd 14 grains. This powder, melted without any flux, produced 12 grains of pure copper. The nails loſt 8 grains in the water, and were, in ſeveral places, cover'd with a ſolid lamina of pure copper. The water, in which the nails lay, after being filtrated and evaporated, afforded a green vitriol, which in every reſpect reſembled *ſal Martis*, and produced the ſame effects, when diſſolved, and mix'd with any aſtringent tincture.

Experiment 5.

From the ſpring-water treated in the ſame manner, I obtain'd a blue vitriol, the baſis of which is copper.

From all theſe experiments it appears, that a mineral acid is the active quality in this water; which being diffuſed thro' the copper ore, unites itſelf with that metal, and forms a vitriol, which is diſſolved by the water, and remains ſuſpended in it, till it meets with iron in the pits, by which this acid is more ſtrongly attracted than by the copper; there-

fore

fore it quits the copper, corrodes the iron, and changes it into a vitriol, which is again diſſolv'd, and carried off in the ſtream continually flowing from the pits; whilſt the copper, deſerted by the acid, falls, by its ſpecific gravity, to the bottom of the pits.

By this account it is evident, that this admirable proceſs is a ſimple precipitation of the copper, by means of the iron. Hence it has been improperly called a tranſmutation of iron into copper. But, leſt any difficulty ſhould ſtill remain, concerning the conſumption of the large quantities of iron put into the pits, I ſhall add the following obſervations, to ſhew, that it is diſſolved and carried off in the water.

Obſervation 1. The water in the pits is cover'd with a thick ſcum, occaſion'd by the air bubbles conſtantly riſing, and burſting on the ſurface; which is an evident ſign of the ſolution of the iron.

Obſ. 2. The iron is gradually conſum'd in the pits, and abounds with irregular depreſſions, like old iron: a ſtrong ſymptom of its being corroded by an acid.

Obſ. 3. The chanel of the ſtream running from the pits is furr'd with red oker, which, after being roaſted in a ſtrong fire, was attracted by the magnet. As this oker is only found in the ſtream below the pits, it appears to be part of the iron diſſolv'd in the water.

Obſ. 4. The quantity of copper found in the pits after the iron diſappears, is generally greater than that of the iron when firſt put in: for the proprietors aſſured me, that ſometimes a ton of iron will produce or rather precipitate, a ton and half of copper.

This fact alone would be sufficient to prove, that the iron is not converted into copper; since, according to Sir Isaac Newton's table, the specific gravity of copper is to that of iron as 9000 to 7645.

When I was at this spring in August last, it ran at the rate of 12 ounces every second; and by putting iron into the water of the stream running from the pits, I found, that every ounce contained three grains of copper. Hence by calculation it appears, that 129600 grains of copper are carried off every minute, and consequently 124100 pounds Troy weight in a year; supposing the quantity and quality of the water to continue the same.

Hence we may easily account for the death of the fish, and other phænomena in the river, which receives this vitriolic stream.

In a hot sunny day, when the water is exhaled, the heaps of mold, raised out of the ore-pits, are cover'd with a vitriolic efflorescence: hence, in rainy weather, the water appears like a strong solution of verdegrise.

Whoever is desirous to imitate the process carried on in these pits, may readily gratify his curiosity, by putting pieces of iron into strong solutions of vitriol.

It is a common experiment, to tinge polish'd iron by rubbing it with Roman vitriol; which depends on the cause before-mention'd; *viz.* the acid in the vitriol penetrates the iron, and leaves the copper on the surface. This experiment is also taken notice of by that excellent chemist, and celebrated philosopher, Mr. Boyle, who calls it a *sympathetic precipitation*, in his *Essay on specific Medicines*.

As

As soon as the attraction between the copper and the acid ceases, the mutual attraction between the minute particles of the metal prevails, so as to form large solid masses at the bottom of the pits, nineteen twentieths of which are pure copper. These solid masses are partly occasion'd by the pressure of the incumbent heap of granulated copper, constantly increasing.

Hence we see, that the art of essaying, or separating metals from their ores, chiefly consists in evaporating an acid, which prevents the mutual attraction of the metallic particles: for when the acid is driven off by the violence of fire, the particles fall into their proper sphere of attraction, and assume a solid form.

From what has been offer'd on the theory of this admirable process, several practical hints may be taken to render such springs more profitable; and perhaps an easier method may be discover'd of separating copper from its ore, by precipitation, than by calcination. But such improvements I refer to those more conversant in the practice of metallurgy.

Having, I hope, given a satisfactory account of the effects of this water on iron, and proved, that it is owing to a strong mineral acid, which it contains, I should next inquire into the source of this acid; but am anticipated in that conjectural research by many inquisitive gentlemen, who have penetrated deeper into the bowels of the earth, and discover'd, or rather taken for granted, a vague acid, which, they say, is diffused thro' the whole mineral kingdom, and being united with different mineral substances, forms vitriols of different kinds. This hypothesis appears the more

probable, ſince it is allow'd by modern chemiſts, that all acid ſpirits, tho' extracted from different minerals, produce the ſame effects, and conſequently are of the ſame nature, the ſpirit of ſea ſalt only excepted, which alone diſſolves gold.

Since vaſt quantities of ſulphur are found in this mine, mix'd with the copper-ore, it is probable, that the acid of the ſulphur is attracted by the copper, and formed into a vitriol, which is afterwards diſſolved by the water.

A farther inquiry into the origin of this acid, I leave to thoſe curious gentlemen, who have ſolv'd the phænomena of volcano's, earthquakes, hot-ſprings, *&c.* from its effects.

The ingenious Dr. Robinſon has lately illuſtrated this doctrine of acids *, by proving, that an acid and light are the ſame thing: for he infers, from Sir Iſaac Newton's philoſophy, that whatever attracts, or is much attracted, is light: therefore an acid is light.

This ſpring perhaps is as remarkable for its medicinal as its metallic qualities. Tho' phyſicians generally reckon copper taken internally poiſonous, yet the miners and other people drink this water frequently, without any ill conſequences. It purges and vomits ſeverely, and is become their ſpecific in ſeveral diſeaſes, particularly in cutaneous eruptions, ariſing either from an alkaline acrimony in the blood, which ſtimulates the ſenſible extremities of the cutaneous arteries, and occaſions a puſtule, or from the irritation of inſects lodg'd in the ſkin; both which cauſes may be removed

* See his Eſſay on the Operations of Medicines.

removed by the ſtrong acid in this water. It is an excellent detergent for ſcorbutic ulcers, as Hoffman juſtly obſerves. It has already perform'd ſeveral remarkable cures of this kind. I have often recommended it in ſuch caſes with ſucceſs, join'd with proper internal medicines; for I am no friend to ſpecifics.

How far the ſucceſs of practice of the miners, who drink it frequently, may be depended on, longer experience muſt determine. Certainly, a great allowance muſt be made for the ſtrength of their conſtitutions, and the inſenſibility of their nerves, conſtantly expoſed to the noxious ſteams of damp pits. I never ventur'd to preſcribe it internally; and as the *materia medica* affords vomits and purges of a more innocent kind, I think it in that reſpect unneceſſary. I have reaſon to imagine, from the effects, which this water had on ſome earth-worms, that it is a very powerful anthelminthic, if cautiouſly given.

Some freſh filings of iron, put in this water, ſoon precipitate all the copper, and make it a ſtrong and agreeable chalybeat. Hence it may be us'd as a ſubſtitute to Spa-water, whoſe virtue depends on the iron. Some prepared filings of iron remain'd eight days in this water; without producing the leaſt alteration. Hence it appears, that this medicine can have but a weak effect, if any at all, in abſorbing acids in the firſt paſſages.

Thus having communicated all, that at preſent occurr'd to me, relating to this ſpring; if theſe obſervations render the hiſtory of it more perfect, and in the leaſt contribute to gratify that laudable curioſity, which you always ſhow, in removing prejudices, rectifying

rectifying mistakes, and encouraging every branch of useful knowlege, I shall think my time well employ'd in this inquiry, which had its rise from your instructive conversation. I am, with the highest respect,

SIR,

London, April 15, 1753.

Your most obedient, and

most humble servant,

John Bond.

XXIX. *A Letter from Dr.* Bevis *to Mr.* James Short, *F. R. S. concerning Mr.* Gascoigne's *Invention of the* Micrometer.

Dear Sir,

Read May 17, 1753.

ALTHOUGH Mr. Townley, in his letter to Dr. Croon, printed in the *Philosophical Transactions*, N° 25, p. 457, has sufficiently made appear, that the invention of the micrometer was Mr. Gascoigne's, and that he applied it to measuring small angles in the heavens, and for settling the moon's parallax, long before Messieurs Auzout and Picard thought of any such matters; yet are the French astronomers at every turn for giving it to these their countrymen, without so much as once mentioning the name of Mr. Gascoigne.

No sooner had the late Dr. Derham restor'd the application of telescopic sights to quadrants to its true author Mr. Gascoigne, than M. de la Hire, who never made the doctor any reply on that head, took occasion,

ſion, in the memoirs of the Royal Academy of Sciences for 1717, to aſcribe this contrivance of the micrometer to M. Auzout, in conjunction with M. Picard; alleging, for proof, an extract of a letter, dated Dec. 28, 1666, from M. Auzout to M. Oldenburg, and printed in *Phil. Tranſ.* N° 21. Several others have ſince copied M. de la Hire's aſſertion, and laſt of all, M. Bouguer, in the memoirs of the Royal Academy of Sciences for 1748, lately publiſh'd, wherein he deſcribes an inſtrument, which he calls an heliometer; the contrivance whereof ſeems in every reſpect the ſame as that ſent about ten years ago to the Royal Society, by Servington Savery, Eſq;.

I have now before me the copy of a letter of Mr. Gaſcoigne to Mr. Oughtred, which I made myſelf from the original, written in 1640-1; which original was in the poſſeſſion of the late William Jones, Eſq; F. R. S. and is now in the library of the right honourable the Earl of Macclesfield. It conſiſts of ſeveral ſheets of paper, all about his invention for meaſuring ſmall angles to ſeconds; where he not only gives the geometrical and optical principles of his contrivance, and the conſtruction of the inſtrument, but alſo a ſeries of obſervations actually taken therewith; ſome of which I ſhall tranſcribe.

			′	″
1640 Aug. 5.	Jupiter's diameter	. .	0	51
	Mars's	. .	0	38
Dec. 24.	Mars's	. .	0	25
	Venus's	. .	0	25

		′	″	
1640 Aug. 25.	Moon's ſemidiam.	15	17	h. 8 p. m.
Sept. 19.	. . .	15	11	

1640 Oct. 9	16 36	h. 8 *p. m.*
10	16 36	
27	15 38	h. 7 *p. m.*
29	15 41	
30	15 43	
31	15 49	

These may suffice to prove, that Mr. Gascoigne's micrometer was not a mere thing in embryo, but brought to a good degree of perfection above 40 years before that of the French gentlemen was ever so much as mention'd. I am,

SIR,

Red-lion street, Clerkenwell, May 10, 1753.

Your very humble servant,

J. Bevis.

XXX. *Observations of the Transit of* Mercury *over the Sun*, May 6, 1753; *by Mr.* J. Short, *F. R. S.*

Read May 17, 1753.

THE instrument prepared for these observations was a reflecting telescope, of two feet focal length, of the Gregorian form, magnifying about 65 times, and so constructed in its machinery as to move in a plane parallel to the horizon, and also, when required, to move in a plane parallel to the equator.* This telescope had two eye-pieces, each a combination of two glasses, *viz.* one eye-piece for the horizontal motion, with wires at right angles to one another, the wires being between the

the glaſſes, and one of the wires placed parallel to the horizon, and conſequently the other was vertical; the other eye-piece was alſo a combination of two glaſſes, and adapted to a micrometer, the glaſſes being placed between the wires of the micrometer and the eye of the obſerver, and was to be uſed when the teleſcope moved in a plane parallel to the equator.

Mr. Short's houſe, in Surry-ſtreet, being ſo ſituated as not to ſee the ſun at riſing, the Rev. Dr. Birch, Secretary of the Royal Society, was pleaſed to allow us the uſe of his leads on his houſe in Norfolk-ſtreet, from whence we ſhould be enabled to ſee the ſun ſoon after he roſe. We choſe the doctor's leads, that we might not be far from Mr. Short's clock, which was eaſily within call, in order to compare a ſecond-watch, which we made uſe of, with the clock, at every obſervation.

About half an hour after 4 o' clock in the morning of the 6th of May, Dr. Bevis and Mr. Short went to Dr. Birch's houſe, where the following obſervations were taken with the above reflecting teleſcope moving parallel to the horizon, and the eye-piece with the wires at right angles; Dr. Bevis obſerving, and Mr. Short writing down the times.

By Mr. Short's clock.

h	′	″	I.
5	20	25	☉ upper limb at the horizontal wire.
	20	29	☉ preceding limb at the vertical wire.
	22	$9\frac{1}{2}$	☿ center at the vertical wire.
	22	33	☿ center at the horizontal wire.
	23	19	☉ ſubſequent limb at the vertical wire.
	24	0	☉ under limb at the horizontal wire.

II

By Mr. Short's clock.

II.

′	″	
25	24	☉ upper limb at the horizontal wire.
25	27	☉ preceding limb at the vertical wire.
27	5½	☿ center at the vertical line.
27	34	☿ center at the horizontal wire.
28	17½	☉ subsequent limb at the vertical wire.
29	0½	☉ lower limb at the horizontal wire.

III.

31	45	☉ upper limb at the horizontal wire.
31	55½	☉ preceding limb at the vertical wire.
33	32	☿ center at the vertical wire.
33	52½	☿ center at the horizontal wire.
34	47	☉ subsequent limb at the preceding wire.
35	20½	☉ under limb at the horizontal wire.

IV.

36	44½	☉ upper limb at the upper wire.
36	54	☉ preceding limb at the vertical wire.
38	29	☿ center at the vertical wire.
38	49¼	☿ center at the horizontal wire.
39	45½	☉ subsequent limb at the vertical wire.
40	18	☉ under limb at the horizontal wire.

V.

41	8	☉ upper limb at the horizontal wire.
41	25	☉ preceding limb at the vertical wire.
42	57½	☿ center at the vertical wire.
43	13	☿ center at the horizontal wire.
44	16	☉ subsequent limb at the vertical wire.
44	39	☉ under limb at the horizontal wire.

VI.

47	45½	☉ upper limb at the horizontal wire.
47	52½	☉ preceding limb at the vertical wire.

5

By Mr. Short's clock.

h	′	″	
5	49	23	☿ center at the vertical wire.
	49	52	☿ center at the horizontal wire.
	50	53½	☉ ſubſequent limb at the vertical wire.
	51	19	☉ under limb at the horizontal wire.

VII.

	55	44¾	☉ upper limb at the horizontal wire.
	55	51½	☉ preceding limb at the vertical wire.
	57	19¾	☿ center at the vertical wire.
	57	48	☿ center at the horizontal wire.
	58	44	☉ ſubſequent limb at the vertical wire.
	59	15¼	☉ under limb at the horizontal wire.

VIII.

6	1	29	☉ upper limb at the horizontal wire.
	1	37¼	☉ preceding limb at the vertical wire.
	3	4	☿ center at the vertical wire.
	3	32½	☿ center at the horizontal wire.
	4	31¼	☉ ſubſequent limb at the vertical wire.
	4	58	☉ under limb at the horizontal wire.

IX.

	5	31½	☉ upper limb at the horizontal wire.
	5	39½	☉ preceding limb at the vertical wire.
	7	5	☿ center at the vertical wire.
	7	33¾	☿ center at the horizontal wire.
	8	33	☉ ſubſequent limb at the vertical wire.
	9	1½	☉ under limb at the horizontal wire.

X.

	13	42¼	☉ upper limb at the horizontal wire.
	13	45	☉ preceding limb at the vertical wire.

By Mr. Short's clock.

h	′	″	
6	15	8	☿ center at the vertical wire.
	15	41	☿ center at the horizontal wire.
	16	39	☉ ſubſequent limb at the vertical wire.
	17	8½	☉ under limb at the horizontal wire.

XI.

18	17	☉ upper limb at the horizontal wire.
18	22½	☉ preceding limb at the vertical wire.
19	44½	☿ center at the vertical wire.
20	17½	☿ center at the horizontal wire.
21	17	☉ ſubſequent limb at the vertical wire.
21	44½	☉ under limb at the horizontal wire.

About half an hour after 6, Dr. Bevis and Mr. Short went to Mr. Short's houſe, the ſun then ſhining into his windows, in order to be near the clock, where the following obſervations were made in the ſame manner as before, Dr. Bevis obſerving, and Mr. Short writing down the times, in the preſence of the right honourable James Earl of Morton, a fellow of this Society.

By Mr. Short's clock.

XII.

h	′	″	
7	1	32	☉ upper limb at the horizontal wire.
	1	43½	☉ preceding limb at the vertical wire.
	2	50	☿ center at the vertical wire.
	3	24½	☿ center at the horizontal wire.
	4	39	☉ ſubſequent limb at the vertical wire.
	4	57	☉ under limb at the horizontal wire.

XIII.

By Mr. Short's clock.

XIII.

h	′	″	
7	6	35½	☉ upper limb at the horizontal wire.
	6	41½	☉ preceding limb at the vertical wire.
	7	45½	☿ center at the vertical wire.
	8	28½	☿ center at the horizontal wire.
	9	35½	☉ subsequent limb at the vertical wire.
	10	0½	☉ subsequent limb at the horizontal wire.

The telescope, which hitherto moved parallel to the horizon, was now altered, to move parallel to the equator, and the eye-piece with the micrometer was applied, about half an hour after seven o' clock, Mr. Short observing, and Dr. Bevis writing down the times of observation.

h	′	″	
7	31	33	distance between the center of ☿ and ☉'s under limb = 19 rev. 6 parts of the micrometer.
	36	7½	☉'s preceding limb at the horary wire.
	36	56½	☿ center at the horary wire.
	37	56	☿ center distant from the ☉'s upper limb = 34 rev. 14 parts.
	41	33	☉'s preceding limb at the horary wire.
	42	21½	☿ center at the horary wire.
	43	21	☿ center distant from the ☉ upper limb = 34 rev. 23 parts.
	44	34	☉ preceding limb at the horary wire.
	45	21½	☿ center at the horary wire. At the same time ☿ distance from ☉ upper limb = 34 rev. 29 parts.

S

h	′	″	
8	4	25	☉ preceding limb at the horary wire.
	5	7	☿ center at the horary wire. At the ſame time ☿ diſtance from ☉ upper limb = 35 rev. 29 parts.
	6	$37\frac{1}{2}$	☉ ſubſequent limb at the horary wire.
	16	$25\frac{3}{4}$	☉ preceding limb at the horary wire.
	17	6	☿ center at the horary wire. At the ſame time ☿ diſtance from ☉ upper limb = 36 rev. 19 parts.
	18	39	☉ ſubſequent limb at the horary wire.
	44	$1\frac{1}{2}$	☉ preceding limb at the horary wire.
	44	$35\frac{1}{2}$	☿ center at the horary wire. Preſently after ☿ diſtance from ☉ upper limb = 37 rev. 28 parts.
	46	14	☉ ſubſequent limb at the horary wire.

The ſun's diameter perpendicular to the equator was taken in the micrometer of the reflecting teleſcope at $7^h\ 25'$, and found to be = 53 rev. 13 parts, = 31′ 50″.

The ſun's diameter perpendicular to the equator was taken in the micrometer of the 12 foot refracting teleſcope (which belonged to the late Mr. Graham) at about half an hour after 9 o'clock, and found to be equal to 51 rev. 14 parts, = 31′ 51″.

Note, The reflector's micrometer has 35 parts in one revolution of the ſcrew; and the refractor's micrometer has 37 parts in one revolution of the ſcrew.

Mr.

Mr. Short obſerved the laſt internal contact of mercury with the ſun, with a four foot focal length reflector, magnifying about 135 times, at $10^h\ 5'\ 7''$ by the clock, uncertain to 2 or $3''$; and the total egreſs at $10^h\ 7'\ 42''$ by the clock; uncertain to 5 or $6''$, the air then undulating thro' thin clouds.

Dr. Bevis obſerved the laſt internal contact with a two foot focal length reflector, magnifying about 65 times, at $10^h\ 5'$, and the total egreſs at $10^h\ 7'\ 38''$, by the clock.

Mr. Siſſon, at Beaufort-Buildings in the Strand, obſerved the total egreſs at $10^h\ 7'\ 43''$, by Mr. Short's clock, thro' a five foot refracting teleſcope.

Mr. Bird, at his houſe in York-Buildings, obſerved the laſt internal contact at $10^h\ 4'\ 57''$; and the total egreſs at $10^h\ 7'\ 43''$, by Mr. Short's clock, thro' a nine inch focal length reflector.

Mr. Smeaton in Furnival's-Inn-Court, Holborn, obſerved the total egreſs at $10^h\ 8'\ 30''$, by Mr. Short's clock, thro' a ſix foot refracting teleſcope. —— He ſuſpects his time ſome ſeconds too late, a cloud having juſt paſſed off the ſun, when he perceived Mercury was gone.

Mr. Canton, in Spital-ſquare, obſerved the total egreſs at $10^h\ 8'\ 12''$, mean time, thro' a reflecting teleſcope, three feet focal length.

Mr. Short's houſe in Surrey-ſtreet is $26''$ of time weſt of the royal obſervatory at Greenwich.

N. B. Mr. Short's clock, by which theſe obſervations were made, was found to be $28''$ ſlower than mean time: but, for more ſatisfaction, here are the ſun's paſſages over the meridian, taken by the tranſitory.

		h	′	″
May 7,	Sun's preceding limb paſſed the meridian at - -	11	54	38
	Subſequent limb paſſed the mer.	11	56	51½
May 8,	Sun's preceding limb paſſed the meridian, at - -	11	54	34½
	Subſequent limb paſſed the meridian, at - -	11	56	47½

N. B. This clock, by repeated obſervations, was not found to have varied above 1″ ſince the 22 of February laſt to the day of obſervation.

In the obſervatory of the right honourable the Earl of Macclesfield, at Shirburn-Caſtle in Oxfordſhire, the total egreſs was obſerved at 10h 8′ 11″, apparent time. Latitude of Shirburn-Caſtle is 51° 39′ 25″, and its longitude is 4′ 0″ of time, weſt of Greenwich obſervatory.

XXXII. *An Account of a Treatise, presented to the* Royal Society, *intituled,* " Letters " concerning Electricity; in which the " lateft Difcoveries upon this Subject, and " the Confequences which may be de- " duced from them, are examined; by " the Abbé *Nollet*, Member of the Royal " Academy of Sciences of *Paris*, Fellow " of the Royal Society, of the Inftitute of " *Bologna*, &c." *extracted and tranflated from the* French, *by Mr.* William Watfon, *F. R. S.*

Read May 17, 1753.

THE work before us contains 262 pages in 12mo, exclufive of the preface, and four copper plates, reprefenting feventeen figures.

This treatife is the production of a great mafter upon the fubject of electricity: he has already publifhed two volumes exprefsly thereupon, befides feveral memoirs among the works of the Royal Academy of Sciences at Paris. For feveral years he has done me the honour of being my correfpondent, and I have communicated feveral valuable papers from him to the Royal Society.

The difcoveries made in the fummer of the year 1752 will make it memorable in the hiftory of electricity. Thefe have opened a new field to philofo-

phers, and have given them room to hope, that what they have learned before in their muſeums, they may apply, with more propriety than they hitherto could have done, in illuſtrating the nature and effects of thunder; a phænomenon hitherto almoſt inacceſſible to their inquiries.

But to make the moſt certain advantage of theſe new diſcoveries, we ſhould confine ourſelves to facts; and if we do draw conſequences from them, they ſhould be immediate and neceſſary ones; for, whenever our diſcoveries ſeem to promiſe to be uſeful and important, we are apt to hope and expect great ſucceſs from them: we muſt therefore be careful to reſtrain our imagination, or we ſhall fall into error.

Theſe conſiderations have induced our author to examine with care, what may truly be concluded from the experiments propoſed by Mr. Franklin of Philadelphia, and ſince carried into execution in France, and elſewhere, in relation to the electricity of the clouds during a ſtorm; by weighing every circumſtance, and comparing the greatneſs of the effects, which have been had in view, with the more than apparent inſufficiency of the means, which have been employed to produce them. He thinks, he ſees clearly, that the conſidering the electriſation of pointed bodies as a proof of leſſening the matter of thunder, is abuſing a real diſcovery to flatter ourſelves with a vain hope; and it is chiefly to diſſipate this error, if it yet ſubſiſts, that determined our author to print, in the work before us, ſome reflexions, which he had made at firſt only for himſelf, and a few perſons, to whom he was deſirous of communicating his opinion.

Mr.

Mr. Franklin's treatiſe upon electricity contains a great many very curious experiments; but the deductions from them being different from thoſe, which the Abbé Nollet has given upon the ſame ſubjects, it might be imagined, if he were ſilent upon this head, that he had given up his former opinions. The honour, which the Royal Academy of Sciences has done them, in publiſhing them in their Memoirs, and the kind reception, which the public has given them, has obliged him to re-examine theſe opinions, and to undertake their defence; more eſpecially as he ſees, that he has powerful reaſons to ſupport them. This has alſo been a motive for the preſent publication, which our author is deſirous ſhould be conſidered, leſs as a criticiſm upon Mr. Franklin's doctrine, than as a defence of his own.

In ſome parts of theſe letters, our author mentions an electricity, which is very often, and perhaps always, in our atmoſphere, when there is no appearance of thunder. He ſpeaks of this, as if he only ſuſpected it, and, in a manner, as if it wanted confirmation. He was then unacquainted with ſome deciſive experiments made upon this ſubject by Dr. le Monnier*, at St. Germain-en-laye, and which have been juſt publiſhed. He now conſiders, as a thing certain, that electricity is a very common meteor, which may manifeſt itſelf, when the weather is moſt ſerene;

* In a memoir read to the Royal Academy of Sciences at Paris, Nov. 15, 1752.

ſerene; and that thunder is, ſtrictly ſpeaking, only one of its modifications, which renders it more ſenſible to us.

The Abbé Nollet's treatiſe contains nine letters; ſix of which are addreſſed to Mr. Franklin, one to Mademoiſelle Ardinghelli, who, when only ſixteen years old, tranſlated Dr. Hales's treatiſe of Hæmaſtatics into Italian, and added thereto ſome very ingenious remarks; one to Mr. Jallabert of Geneva, and one to Mr. Boze of Wittemberg: To theſe are added ſome experiments in electricity, made in ſupport of opinions, laid down in this work, in the preſence of Meſſieurs Bouguer, de Montigny, de Courtivron, d'Alembert, and le Roi, who were appointed by the Royal Academy of Sciences for that purpoſe.

In the firſt letter our author gives his correſpondent Mademoiſelle Ardinghelli an account of the diſcoveries in electricity in the year 1752; among which he takes particular notice of the experiment made on May 10, at Marly-la-ville, in conſequence of Mr. Franklin's hypotheſis; wherein pointed non-electrics, ſupported by electrics *per ſe*, gave manifeſt ſigns of electricity during a thunder-ſtorm. This experiment, in the letters to Mr. Collinſon, Mr. Franklin had propoſed, but, as far as may be judged, had not then carried into execution. The experiment of Marly-la-ville was ſoon after verified by Dr. le Monnier at St. Germain-en-laye, who found further, firſt, that the like effects were produced, whether the iron rods were pointed, or not; and that it was indifferent, whether their poſition was horizontal, or not. Secondly, that thunder electriſed not only iron, but alſo

wood,

wood, living bodies, and other electrifable fubftances. Thirdly, that it was not abfolutely neceffary to place thefe bodies at the tops of buildings; and that it was fufficient for them to be placed about four feet from the ground in an open fituation, and at fome diftance from large buildings. Fourthly, that bodies electrifed in this manner produced the like phænomena with thofe electrifed by glafs after the ufual manner. It was afterwards difcovered, that electrifable bodies, thus difpofed in open air, were fometimes electrifed under thick clouds, but without thunder, lightning, or even without rain or hail.

The Abbé Nollet recommends, that thefe experiments fhould be made with circumfpection, as he has been informed by letters from Florence and Bologna, that thofe, who have made them there, have had their curiofity more than fatisfied by the violent fhocks, which they have fuftained, in drawing off the fparks from an iron bar electrifed by thunder. One of thefe in particular fays, that once, as he was endeavouring to faften a fmall chain, with a copper ball at one of its extremities, to a great chain, which communicated with the bar at the top of the building, in order to draw off the electrical fparks by means of the ofcillations of this ball, there came a flafh of lightning, which he did not fee, but which affected the chain with a noife like wild-fire. At that inftant, the electricity communicated itfelf to the chain of the copper-ball, and gave the obferver fo violent a commotion, that the ball fell out of his hands, and he was ftruck backwards four or five paces. He never had been fo much fhocked by the experiment of Leyden.

From

From the experiment at Marly-la-ville, and thoſe which have been made ſince, have been drawn two conſequences; one, that the matter of thunder, and that of electricity, are one and the ſame: the other, that, by the means of pointed iron rods, one might, without its doing any harm, draw off all the fulminating matter from a ſtormy cloud. But our author has ſhewn, that bodies being pointed are not abſolutely neceſſary; and is deſirous, we ſhould not too haſtily believe, that miſchiefs ariſing from thunder may be averted by the apparatus propoſed. He thinks the means vaſtly too ſmall for the greatneſs of the cauſe.

Our author's firſt letter to Mr. Franklin is an introduction to the five ſubſequent ones.

The ſecond letter treats of the nature of the electric matter. In this its analogy with fire is conſidered and proved; and our author takes notice, that Mr. Franklin, he imagines, who has certainly made ſome important diſcoveries into the properties of electricity, cannot but be diſſatisfied with the editors of his work, for publiſhing, "that he exhibited to "our conſideration an inviſible ſubtil matter, diſſe-"minated throughout all nature, *&c.* which had hi-"therto eſcaped our obſervations." The latter part of which aſſertion is not ſtrictly true; as the conſidering the matter of fire, and that of electricity, to be one and the ſame, is a fundamental principle of what both the Abbé Nollet and myſelf formerly publiſhed upon this ſubject.

The third letter to Mr. Franklin contains ſeveral proofs, that glaſs is not impermeable to the electric

matter.

matter. Some of the experiments of our author upon this ſubject I heretofore did myſelf the honour to lay before you; and they are in my opinion fully concluſive.

The fourth letter to Mr. Franklin relates to ſeveveral phænomena of the experiment of Leyden. In this letter it is examined, whether the effects of this experiment proceed from the glaſs phial, or from the non-electrics contained therein; and experiments are produced to prove, that the power of giving a ſhock in an electriſed phial of water, proceeds from the water in the phial, and not from the phial itſelf, as Mr. Franklin imagines. In this letter likewiſe is an examination of Mr. Franklin's opinion, that, in the charged phial, as much fire as is received by one of its ſurfaces is loſt by the other.

The fifth letter to Mr. Franklin is in relation to the power of pointed non-electric bodies drawing off and throwing off electrical fire, at a much greater diſtance than obtuſe bodies do of the ſame kind. Our author thinks, that Mr. Franklin has attributed more power to pointed bodies, than, upon experiment he finds to be true.

The ſixth letter to Mr. Franklin is upon the analogy of thunder with electricity. This is a fact at preſent ſo well eſtabliſhed, as to admit of no doubt. But our author cannot agree with Mr. Franklin in his opinion, "that thunder is at preſent in the power of "men, and that we are able to diſſipate it at our "pleaſure: that an iron rod (ſuch a one as Mr. "Franklin has directed, and ſuch a one as has been "made uſe of) is ſufficient to diſcharge of all its fire

"a

" a ſtormy cloud againſt which it is directed." For his part, he confeſſes, that he cannot believe it; firſt, becauſe he ſees too great a diſproportion between the effect and the cauſe: ſecondly, becauſe the principle, which is given us to ſupport this opinion, is not ſufficiently eſtabliſhed. He can hardly think, that the fulminating matter, contained in a cloud, capable of covering a great city, can be drawn off in a few minutes by a pointed bar, as thick as your finger. If even a number of theſe placed upon the tops of eminencies were only neceſſary to prevent the effects of thunder, would not the vanes and croſſes at the tops of our ſteeples have been ſufficient to procure us this advantage? Theſe buildings however, in all times, have not been exempted from the miſchiefs of thunder. He deſpairs of our weak efforts ever being able to diſarm the heavens.

Our author here gives us the repreſentation and deſcription of his apparatus for electriſing during the thunder: it differs in nothing eſſential from thoſe, which we uſed laſt year.

In this letter are likewiſe conſider'd the validity of Mr. Franklin's hypotheſis of electric and non-electric clouds; the former ariſing from the ſea, the latter from the land; their operation, upon their approaching one near the other; the difference, according to Mr. Franklin, between electrical and common fire; and ſeveral other parts of Mr. Franklin's doctrine.

The eighth letter is addreſſed to our worthy brother Profeſſor Jallabert of Geneva; and, among other curious particulars, inſerts part of a letter, which our author had received from Mr. Jallabert, giving an

account of an experiment, which Mr. Jallabert had ſome time ſince made at the water-works at Geneva. An account of this experiment was communicated by myſelf to the Society; and it has near relation to the experiment, which we made here in electriſing the river Thames ſix years ago. Mr. Jallabert conſults the Abbé Nollet in relation to the ſolution of the phænomena of this experiment; and the Abbé does me the honour now to give the ſame ſolution thereto, which I firſt gave to a ſimilar experiment of Dr. le Monnier's, and laid before the Society in January * 1746, and ſince applied upon other occaſions in illuſtrating the electrical circuit.

The ninth letter is addreſſed to Mr. Boſe, profeſſor of mathematics and philoſophy at Wittemberg; and is in anſwer to one of Mr. Boſe, in which this gentleman expreſſes himſelf ſurpriſed, that ſo many ages have paſſed, without it having been diſcovered, that thunder electriſes bodies; ſince it depends upon an experiment ſo ſimple, and which it is hardly poſſible to fail in, when you deſire to repeat it under proper circumſtances.

Upon this our author obſerves, that people in general only ſee the facts, and are ignorant of, or do not conſider, the means, by which philoſophers arrive at them; nor perceive the circumſtances, without which theſe phænomena could never have been made known to us; and that Mr. Boſe will ceaſe to be ſurpriſed, as he is ſo well verſed in theſe phænomena, when he reflects upon what our author offers.

To

* See *Phil. Tranſ.* Vol. XLIV. p. 388.

To make the experiment in queſtion, it is neceſſary that bodies ſhould be ſupported by glaſs, ſilk, or reſin, without touching any thing elſe communicating with what we now call non-electrics; without which, the ſigns of electricity, which are ſought for, cannot manifeſt themſelves.

To this experiment, therefore, a previous knowlege is required, of inſulating bodies to be electriſed; but where is the man who was acquainted with this fact thirty years ago? Before that period, it was not even gueſſed at by any one.

Since Mr. Gray diſcovered, that bodies muſt be inſulated, to communicate to them a perceptible electric virtue, to what purpoſe could we ſet up iron bars under a ſtormy cloud? This thought could not have happened, but to thoſe who had taken notice of the analogy between lightning and electricity, and upon whom this idea had made a ſtrong impreſſion. And no one could think ſeriouſly upon this analogy, but ſince the diſcovery of the experiment of Leyden, that is, ſince the year 1746. Before that time the electriſation of bodies by thunder could not have been perceived, bu by an accident very difficult to meet, on account of the conditions requiſite.

Nevertheleſs it may be urged, that bodies, being really electriſed, have ſhewn themſelves in all ages *, as

* I formerly took notice, that the electrical attraction had been obſerved ſo early, as to be mentioned by Theophraſtus (ſee *Phil. Tranſ.* Vol. XLIV. p. 732); ſo its luminous appearance, though only conſidered as a meteor, is mentioned by Plutarch, in the life of Lyſander. Pliny, in the ſecond book of his natural hiſtory, chap. 37.

as hiſtorians both ancient and modern have made formal mention thereof. But to this it may be replied, that it was not enough to know the fact, unleſs people were enough acquainted with it to take it for what it really was; that is, the electric virtue: for without that, obſervations of this kind could have very little weight with

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37, calls theſe appearances ſtars; and tells us, not only that they ſettled upon the maſts, and other parts of ſhips, but alſo upon mens heads: 'Exſiſtunt (ſays that hiſtorian) ſtellæ et in mari terriſque. 'Vidi nocturnis militum vigiliis inhærere pilis pro vallo fulgorem 'effigie ea: et antennis navigantium, aliiſque navium partibus, 'ceu vocali quodam ſono inſiſtunt, ut volucres ſedem ex ſede mu-'tantes: . . . . geminæ autem ſalutares, et proſperi curſus 'prænunciæ; quarum adventu, fugari diram illam ac minacem, 'appellatamque Helenam, ferunt. Et ob id Polluci et Caſtori id 'numen aſſignant, eoſque in mari deos invocant. Hominum quo-'que capiti veſpertinis horis, magno præſagio circumfulgent." But (adds he) all theſe things are 'incerta ratione, et in naturæ 'majeſtate abdita.'

Seneca, in his Natural Queſtions, chap. 1. takes notice of the ſame phænomenon. 'Gylippo (ſays he) Syracuſas petenti viſa eſt 'ſtella ſuper ipſam lancem conſtitiſſe. In Romanorum caſtris viſa 'ſunt ardere pila, ignibus ſcilicet in illa delapſis.'

Cæſar *de Bello Africano*, cap. 6. *edit. Amſtel.* 1686. We here find them attending a very violent ſtorm. 'Per id tempus fere 'Cæſaris exercitui res accidit incredibilis auditu; nempe Virgili-'arum ſigno confecto, circiter vigilia ſecunda noctis, nimbus cum 'ſaxea grandine ſubito eſt exortus ingens. . . . Eadem nocte 'V legionis pilorum cacumina ſua ſponte arſerunt.'

Livy, chap. 22. mentions two ſimilar facts: 'In Sicilia militi-'bus aliquot ſpicula, in Sardinia in muro circumeunti vigilias 'equiti, ſcipionem, quem in manu tenuerat, arſiſſe, et litora cre-'bris ignibus fulſiſſe.'

Theſe appearances are called by both French and Spaniards inhabiting the coaſts of the mediterranean, St. Helme or St. Telme's fires;

with any perſon engaged in the inquiry. At preſent, indeed, when we know, from the experiment of Marly-la-ville, that a ſtormy cloud is a great electric maſs, the action of which extends itſelf ſenſibly even to bodies, which are upon the ſurface of the earth, we muſt agree, by reflecting on them, that the lights, which have been ſeen upon the croſſes placed on the tops of ſeveral ſteeples, thoſe, which the Roman ſoldiers ſaid they had obſerved at the end of their pikes, and thoſe lambent flames, which appear upon the maſts of ſhips, which mariners call St. Helmo's fire, are ſo many electrical phænomena. But until the moment that this experiment was made, which open'd our eyes with regard to the poſſibility and nature of theſe marvellous effects, theſe appearances were regarded either as popular illuſions, or falſe prodigies, or even as luminous vapours, which might be ranged in the claſs of *phoſphori*. Moreover, as theſe were ſeen but ſeldom, if ever we had been tempted to attribute them to the influence of ſtormy clouds, we might have been diſſuaded therefrom, by conſidering the little agreement there is, between the rarity of

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fires; by the Italians, the fires of St. Peter and St. Nicholas, and are frequently taken notice of by the writers of voyages.

If ſome late accounts from France are to be depended upon, we are informed, that at Plauzet it has been obſerved for time immemorial; and M. Binon, the curé of the place, bears his teſtimony of the truth, that, for twenty-ſeven years, which he has reſided there in that capacity, in great ſtorms, accompanied with black clouds, and frequent lightnings, the three pointed extremities of the croſs of the ſteeple of that place appear ſurrounded with a body of flame; and that, when this phænomenon has been ſeen, the ſtorm was no longer to be dreaded, and calm weather returned ſoon after.

of theſe effects, and the frequency of the cauſes, which might produce them.

We ſee, therefore, how important it is to deſcribe exactly the phænomena we obſerve: otherwiſe, how long may it be, before we can deduce any real inſtruction from thoſe, which we have been informed of in a negligent and ſuperficial manner? We have heard all our lives of St. Helmo's fire, of thoſe which the antients call Caſtor and Pollux, and of the comazants of our mariners. But, from what we have had related to us, and from what we have read, who could have been prevailed upon to range them with electrical phænomena? We have heard them repreſented, as thin lambent ſhining lights, a kind of phoſphoreal vapour: but there is a paſſage in the memoirs of the Count de Forbin, quoted by our author, wherein mention is made of St Helmo's fire; which if any one, well verſed in the phænomena of electricity, had carefully attended to and conſidered a few years ago, he might have prognoſticated ſucceſs to Mr. Franklin, when he propoſed his experiment upon thunder. "In the night (ſays the author of "thoſe memoirs) on a ſudden it became exceedingly dark, and thunder'd and lightened moſt dreadfully. As we were threatened with the ſhip's being torn to pieces, I ordered the ſails to be taken in: We ſaw, upon different parts of the ſhip, above thirty St. Helmo's fires: Among the reſt, there was one upon the top of the vane of the main-maſt, which was more than a foot and half in height. I ordered one of the ſailors to take it down: When this man was on the top, he heard this fire; its noiſe reſembled that of fired wet gunpowder: I

"ordered

" ordered him to lower the vane, and come down;
" but ſcarce had he taken it from its place, but
" the fire left it, and fixed itſelf upon the top of
" the mainmaſt, from which it was impoſſible to
" remove it; and continued there a conſiderable time,
" until it went out by little and little, *&c.*"

If all the authors, who have taken notice of St. Helmo's fire, had ſpoken of it as this juſt quoted, philoſophers might have reproached themſelves for its having been ſo long before they had a juſt idea thereof, and for their not having ſhewn the principle upon which it depended. But how few hiſtorians are there, who could have related this fact with circumſtances ſo proper to put us in a right train, as thoſe juſt mentioned?

" And here I cannot but obſerve, as I am con-
" vinced, that the matter of thunder and that of
" of electricity are one and the ſame, how vaſt an
" idea muſt the attending to the before-mentioned
" paſſage excite in the mind of perſons, accuſtomed
" to the phænomena of electricity? How immenſe
" a quantity of it muſt they conceive to have been at
" that time in the atmoſphere ſurrounding the ſhip,
" and within the verge of its action, to furniſh more
" than thirty St. Helmo's fires; the ſame, in fact,
" which we ſee at the ends of our conductors in
" electriſing, one of which was more than a foot and
" half in height? At this time, and under theſe cir-
" cumſtances, the maſts, yards, and every part of
" the ſhip, I conſider as conductors of electricity,
" between the, at this time electriſed, atmoſphere,
" and the ſea: and tho', being of a vegetable nature,
" and, if dry, even of the worſt kind for this pur-

" poſe,

" pofe, they conducted electricity much lefs perfectly " than metal under the like circumftances would " have done, I doubt not, but that they were greatly " inftrumental in averting the danger, with which " the fhip was threatened.

" Upon thefe confiderations, I do not fcruple to " recommend, as Mr. Franklin has done, communi- " cations of metal between the fpindles and iron- " work at the tops of the mafts of fhips, and the fea; " or, which will anfwer the fame purpofe, the bilge " water in the well. This can be liable to little " objection, as the doing it is neither difficult, nor " expenfive; an iron wire, of the thicknefs of a goofe- " quill, conducting electricity more readily than any " piece of timber, however large; and thefe mafts " do it fo much the worfe, as they are of a refinous " nature.

" From attending to thefe phænomena, we every " day fee more and more the perfect analogy (to " compare great things with fmall) between the " highly electrifed glafs jar, in the experiment of " Leyden, and a cloud replete with the matter of " thunder. But more of this poffibly upon fome " future occafion.

" Though the number and continuance of the St. ' Helmo's fires, in the paffage before-mentioned, ' probably tended greatly to preferve the fhip from ' the deftruction, with which it was then threatened, ' yet the caufe may be too great, and come on too ' faft, to be leffened enough by thefe means to avert ' the mifchief. Thus in the account, publifhed in ' the * *Philofophical Tranfactions*, from Captain John

" Waddel,

* Vol. XLVI. p. 111.

" Waddel, his ſhip was almoſt beaten to pieces by
" the thunder and lightning: although, as he expreſſes
" himſelf, there were ſundry large comazants over
" head, ſome of which ſettled on the ſpindles on
" the topmaſt-heads, and burnt like very large
" torches. When this account was written, theſe
" phænomena were only conſidered as the preſages
" or attendants of a ſtorm, and no ſort of inference
" propoſed from them."

But to return to our author: His work cloſes with a ſeries of experiments, intended to demonſtrate the validity of the concluſions exhibited therein. Theſe merit the particular attention of thoſe converſant in theſe matters; but I muſt refer you here to the work itſelf, and only obſerve, that ſome of the experiments are made *in vacuo*, and are of the ſame kind with thoſe which I communicated to the Royal Society in February 1752; and which have been ſince publiſhed in the *Philoſophical Tranſactions* *.

Upon the whole, I think the treatiſe before us a very valuable one, as it gives us the ſtill riper thoughts of an able writer upon a difficult, and, till very lately, an almoſt unknown, ſubject; of one, who, beſides his inquiries into this part of philoſophy, has a great compaſs in the knowlege of nature, and is therefore well qualified to inveſtigate her phænomena.

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* Vol. XLVII. p. 363, et ſeq.

XXXII.

XXXII. *The Number of People in the City of* Briſtol, *calculated from the Burials for Ten Years ſucceſſive, and alſo from the Number of Houſes; by* John Browning, *Eſq*; *of* Barton-hill *near* Briſtol: *Communicated by* Henry Baker, *F.R.S.*

Read Jan. 25, 1753.

The certificates were obtained under the hands of the præcentor of the college, the ſeveral miniſters of the ſeventeen pariſh-churches, the regiſter-keeper of the ſeveral Quakers cemeteries, the ſeveral Anabaptiſts cemeteries, the Jews new-erected cemetery, for ten years, including the year 1741 and 1750. Whereas ſome of the pariſhes within the liberties of the city do extend themſelves beyond the liberties into the counties of Glouceſter and Somerſet, they are diſtinguiſhed by the names of the out-pariſhes. The inhabitants of the ſeveral out-pariſhes being buried within the liberties of the city, muſt of conſequence be brought into the city bill of mortality.

Names of the Parishes.	Burials.
The Cathedral	23
All Saints	58
St. Michael's	637
St. Werburgh's	101
St. James's	2945
St. Mary Redcliff's	1538
St. Thomas's	639
Temple	1398
St. Nicolas's	844
St. Mary Port's	245
St. Peter's	638
St. Philip and Jacob	3661
St. Ewin's	30
St. John Baptift's	538
St. Leonard's	74
St. Stephen's	1234
St. Auguftin's	916
Chrift-church	311
The Quakers Cemeteries	506
The Baptifts Cemeteries	979
The Jews new-erected Cemetery	2
Total Number Burials for Ten Years	17317

$17317 \div 10 = 1731$ Burials in each Year.

Rated

Rated Houses in each Parish.	Number.
All Saints	42
St. Michael's	375
St. Werburgh's	43
St. James's	1020
St. Mary Redcliff's	420
St. Thomas's	209
Temple	211
St. Nicolas	411
St. Mary Port	106
St. Peter's	181
St. Philip and Jacob (large out-parish)	363
St. Ewin's	25
St. John Baptist's	144
St. Leonard's	54
St. Stephen's	375
St. Augustin's	480
Christ-church	160
The Castle Precinct's Ward	247
Total number of rated houses	4866

The latest and most accurate observations demonstrate, that in great cities a twenty-fifth part of the people die yearly.

The yearly amount of the burials at a medium for ten years, $1731 \times 25 = 43275$, the number of inhabitants.

The number of houſes rated to the land-tax, as appears by the rates in the council-houſe, Michaelmas, 1751, . .	4866
As the rates are not always accurately made, and as it is the uſual cuſtom not to rate houſes, which are untenanted, hoſpitals, or alms-houſes, it will be neceſſary to make a very large allowance for theſe deficiencies, eſpecially as many houſes are rated in groſs under the denomination of ſeveral tenements; when they belong to the ſame landlord in all the ſeveral pariſhes, an allowance of 25 *per cent.* will be more than ſufficient	1216
The number of houſes computed in the ſeveral out-pariſhes . . .	1200
Total number of houſes	7282

The uſual number of ſouls allowed to each houſe is ſix.

Houſes

$7282 \times 6 = 43692$ number of inhabit^s^. by the houſes.

43275 number of inhabit^s^. by the burials.

XXXIII. *An Account of the Eclipse predicted by* Thales; *by the Rev.* William Stukeley, *M. D.*

Read May 3, 1753.

WHILST I lived in Lincolnſhire, I was viſited by Mr. Edmund Weaver, who was a very uncommon genius; particularly he had made himſelf a great maſter of aſtronomy, and was ſcarcely to be accounted the ſecond in the kingdom. He compoſed complete tables of the celeſtial motions, which he was very much ſolicited to publiſh; but the world waited for Dr. Halley's.

Theſe tables were ſhewn to Dr. Halley: we may have a notion of their value from what the doctor ſaid thereupon, that he ſuſpected, Mr. Weaver had ſeen his tables. He was well known to, and much eſteemed, by Mr. Martin Folkes. He taught himſelf writing, arithmetic, algebra, ſome ſublime parts of the mathematics, the whole art and ſcience of aſtronomy; as his annual publications ſufficiently evince. He was an inſtance of great merit in obſcurity: he died in a little houſe of his own, ſoon after I removed to London, Dec. 27, 1748. and was buried at Cathorp near Grantham.

Thus much I thought proper to commemorate of this worthy perſon. An intimacy grew up between him and myſelf during that twenty years I lived in the country, nor was it unfruitful; for we often agreeably entertained ourſelves in calculations of aſtronomy, with a view to antient hiſtory. One of them I here produce before the Society, done many years

years ago; but, upon hearing that read on the ſame ſubject from Mr. Coſtard, it put me in mind of it, and I hope it will be an acceptable illuſtration and confirmation of that famous piece of hiſtory, the eclipſe predicted by Thales the Mileſian; which happened in the 603 year before the Chriſtian æra.

I ſhall recite the hiſtory of this matter, as conciſely as I can, from the hiſtorians and writers, tho' they all miſtake the year. But this ſhews the admirable uſe to be made of aſtronomy in aſcertaining matters of hiſtory.

The great king of Babylon, Nebuchadnezzar, was now buſy in executing the vengeance, which God had made him the inſtrument of, upon the nation of the Jews, for their incorrigible wickedneſs and folly. Their king Jehoiachin was carried away captive to Babylon, and kept in priſon 37 years together, till he died.

At this time there was a ſharp war between the Medes and Lydians, of which Herodotus give us an account. Halyattes, father of the famous Crœſus, was now king of the Lydians.

After the Medes had conquer'd all the upper or northern part of Aſia, from the old poſſeſſors the Scythians, they again extended their borders to the river Halys in Leſſer Aſia, the boundary between Cappadocia and Armenia, or between the Lydians and Medes. It was not long before there happened a war between theſe nations, which continued for five years together, with various ſucceſs.

In the ſixth year they engaged each other, with the utmoſt of their ſtrength; intending to make that battle deciſive of the quarrel, that was between them:

but, in the midst of the engagement, whilst the fortune of the day seem'd to hang in an equal balance, there happened a total eclipse of the sun, which overspread both armies with a horrible darkness; insomuch that, being affrighted at such a critical judgment of heaven (as they thought it), both sides put up their swords; and they agreed to refer the controversy between them to two arbitrators. Halyattes, king of Lydia, chose Siennesis, king of Cilicia; Cyaxares, the Median monarch, chose Nebuchadnezzar, now busy in leading the Jews into captivity.

Nebuchadnezzar is by Herodotus called Libynetus. It seems to me, that the letter N, in the beginning of the word, has, in the antient copies of Herodotus, been turned into Λ; and then the words, in two different dialects, are not very different.

These great arbitrators compromised the matter between the contending parties, by making a match between the two royal families; and so restor'd peace and friendship. Astyages, the son of Cyaxares, king of Media, married Ariena, daughter of Halyattes, king of Lydia, of whom, a year after, was born Cyaxares, whom the prophet Daniel calls Darius the Mede. And in that last mention'd year, king Cyaxares gave his daughter Mandane in marriage to Cambyses king of Persia; of whom, the next year, was born the great Cyrus, the founder of the Persian monarchy, whom the prophet Isaiah foretold by name, that he should restore the polity of the Jews, the city of Jerusalem, and the temple, and return the sacred vessels of gold and silver, which Nebuchadnezzar had carried away, and put into his heathen temple at Babylon.

Thus ended this famous quarrel between the Medes and Lydians, thro' the timely event of a total ſolar eclipſe, made ſtill the more eminent, that it was calculated, and foretold to the Ionians by Thales of Miletus, at that time in the 37 year of his age. He was born of Phœnician parents; and there, no doubt, learned his knowlege in aſtronomy. He was the firſt, who brought this ſcience into Greece, and that 300 years after the pretended Chiron of the Argonauts.

It is an invincible argument, that he learned his art; for a whole life is not ſufficient, ſo to obſerve the motions of ſun and moon, as to be able to calculate an eclipſe.

This is the firſt eclipſe, which we have recorded in ſo circumſtantial a manner. Notwithſtanding all this, it is ſtrange to ſee, how the learned have erred about the true year of this memorable affair.

Pliny begins the miſtake, telling us, that it was the fourth year of the XLVIII Olympiad; whereas it was the fourth year of the XLIII. It is not unlikely, the numeral letter V is crept into the original. Clemens Alexandrinus makes it about the fiftieth Olympiad. Dr. Prideaux makes it 5 years too late; Archbiſhop Uſher 2 years. Sir Iſaac Newton gives us the true month and day, but aſſigns the 585 year, as Ricciolus.

I have deſigned the map here exhibited, from my friend Weaver's calculation (Fig. IX.), which will preſent us with a juſt notion of the whole affair. It is a projection of the moon's ſhade, as it paſſed over the earth's ſurface from 20 to 60 degrees of longitude eaſt from London; and from 25 to 50 degrees of

north

north latitude, with the hours, half-hours, and quarters of time, where vertical. This was on the 18 of May in the proleptic Julian ſtyle: in the year of the Julian period 4111, the 603 year before the vulgar æra of Chriſt.

The eclipſe was total 4 minutes and a half, where the battle was fought. The ſhade enters our map, in the deſert of Barca in Africa, ſoon after 9 o' clock in the morning. It traverſes the mediterranean ſea, and iſle of Cyprus; enters Aſia Minor at Cilicia, a little before 11; about half an hour after, it paſſes the city now called Erzerum; near which I ſuppoſe the battle was fought, as being at the boundary between the two kingdoms. It is between the river Halys, and the river Melas, on which was the antient city Melitene. The river Melas runs eaſtward into the Euphrates. At half an hour after 12, the ſhade enter'd upon the Caſpian ſea, and at 1 upon the Kalmuc Tartary.

We ſee here an authentic *parapegma* in antient hiſtory, deduced from aſtronomy: and we ſee a remarkable inſtance, brought about by Divine Providence, of a moſt furious war, terminated by the intervention of an eclipſe. But eclipſes, ſay we, are natural and neceſſary phænomena, conſequent to the eſtabliſhed motions of the celeſtial bodies. True Providence order'd them at the beginning, as well as comets, and earthquakes, and the like portentous appearances, as antiquity rightly denominated them and regarded them: but Providence did not thereby reſtrain its own power and authority, to render them at the ſame time of moral uſe. Providence can ſo over-rule the actions of us men, as to bring them to coincide with theſe fore-ordain'd and neceſ-

ſary motions, ſo as to prove himſelf the GOVERNOR both of the natural and moral world; tho' improv'd philoſophy has given us a juſter notion of theſe matters than the antients had. I wiſh our religious ſentiments may advance, in proportion to our improved philoſophy.

March 16, 1753.

Wm. Stukeley.

XXXIV. *A farther Account of the* Giant's Cauſeway *in the County of* Antrim *in* Ireland, *by the Rev* Richard Pocock, *LL.D. Archdeacon of* Dublin, *and F. R. S.*

Read May 24, 1753.

IN a letter, which I wrote in 1747 to Martin Folkes, Eſq; Preſident of the Royal Society, which was read in January, and printed in the *Philoſophical Tranſactions* for that month, I obſerved, in relation to the Giant's Cauſeway, that there appeared in the Sea-cliffs three ſtrata of pillars between thirty and forty feet high, with ſtrata of a black rock between them; that the cauſeway itſelf was the loweſt of all theſe, extending in a point into the ſea; and that another is ſeen towards the top of the cliff.

Laſt ſummer I took another view of it; I went from Bally-Caſtle, which is about 10 miles to the eaſt of the Cauſeway. When I came two miles to the weſt of Bally-Caſtle, within leſs than a mile of Ballintoy, half a mile to the ſouth of the ſea-cliffs, and about a quarter to the ſouth of the road, I ſaw the ſame

ſame kind of pillars in a low hill; I obſerved both hexagons and pentagons.

The rocks towards the ſea appeared as if they were formed in the ſame manner; but when I came to them, I found it was only common rock in ſeveral ſtrata, and perpendicular joints.

I went on about two miles to a peninſula called Donſeverik, where I ſaw ſome tendency in the rock towards this work of nature; and going about half a mile farther, came to the beginning of the pillars in the ſea-cliff, as I believe, about five miles from the cauſeway: and the ſhore and cliffs being ſhap'd moſtly in little ſemicircular bays, I had many very beautiful views of the upper and middle ſtrata of pillars: in one, particularly, they had much of the appearance of ruin'd portico's one over the other; and turning the little end of a ſpy-glaſs, it appeared ſomething like the ruins of Palmyra, as a view of them is repreſented in a copper plate, publiſhed in the *Philoſophical Tranſactions.*

This wonderful work of nature is continued on in the cliffs for about a quarter of a mile beyond the Giant's Cauſeway.

I ſaw it again in the road to Coleraine, five miles to the weſt of the Cauſeway, in a low hill a furlong to the ſouth of the road, and two miles to the ſouth of the ſea. The pillars here are ſmall; and being about a mile and a half from Ballimagarry, where the earl of Antrim has a ruin'd houſe, lately burnt down, it ſerv'd, as I ſuppoſe, as a quarry for building part of that houſe, in which I ſaw a great number of the ſtones, and particularly one of nine ſides. I ſaw others near two miles farther, to the ſouth of

the road in a low hill, within two miles of Coleraine; ſo that the whole extends for about eleven Iriſh miles, or fourteen Engliſh.

Beyond Coleraine, to the eaſt of Magilligan, I ſaw in the rocks towards the ſea-cliffs, the ſtones in the rocks towards the ſea-cliffs, the ſtones in the hills very regular, appearing at a diſtance much like theſe pillars. This is ſix computed miles beyond Coleraine, and conſequently about ten Engliſh miles from the laſt pillars.

At Fairhead alſo, a high point of land, three miles to the eaſt of Ballycaſtle; towards the top of it, the rock appears as in grand pillars. They ſay it is not in joints, but it has ſomething of the appearance of a grand Gothic piece of workmanſhip.

As I ſpent a week at the Cauſeway, and ſent away by ſea to Dublin as great a variety of the ſtones as I could conveniently get, particularly a large octagon, with the eight large ſtones round it; a pair of leſs, with eight pair, that encompaſs it; two ſmall pentagon pillars, about fourteen inches over, one of them three feet ten inches and a half high, the other five feet ſeven inches; one hexagon pillar, about the ſame ſize, and five feet five inches high; all which I have placed in my garden; ſo I have had an opportunity of conſidering it at leiſure.

It is a black ſtone, weighty and brittle: and I have been informed, that it was tried in a glaſs-houſe, and that it melted with kelp, ſo as to make the black glaſs bottles: which experiment, I have been told, was made by Mr. Dobbs of this kingdom, who is now in London.

Mr.

Mr. Drury, whom I ſhall have occaſion to mention, found in a ſtone of the Cauſeway a rough pebble in the ſhape of an egg, about three quarters of an inch long, and above an inch thick; and when it was poliſh'd, it proved to be a white cornelian. They are from three ſides to nine ſides, frequently encompaſſed with as many ſtones as there are ſides; but many of them have a narrow ſide, which has no ſtone to it, but is filled up with a piece or pieces of ſtone, that ſhall be further explain'd; which pieces, when the ſtones are mov'd, commonly ſeparate, and break off. Some ſtones have two, or three, or more, of theſe ſides: ſo that it is poſſible, a ſtone, that has any number of ſtones round it, may have double the number of ſides: tho' I ſaw none, that I had reaſon to think were of this kind, except ſome, that had probably only three ſtones round them; being hexagons, with three broad ſides, and three very narrow ſides.

Whatever the outward figure of the ſtone is, the concavity or convexity is either circular, or part of a circle; conſequently, as the ſides of the pillars are plain, the part between the inſide circle and the outward figure muſt either be fill'd up (as it is ſeen) by ſtone, which ſometimes ſeparates, as mention'd above, and as will be further explain'd; or by the matter preſſed up from the ſides, as will be more plainly deſcribed. In the former caſe, when the end is convex, this ſtone often comes off all round at the joint, and leaves the convex end as pa t of a ſphere, and the concave as a mould fitting to it.

I have ſome ſtones exactly like a hexagon cut in two, which might be part of a hexagon pillar ſplit; for

for ſometimes a whole pillar appears as ſplit all the way down ; of which there is a remarkable one at the Cauſeway.

In relation to the joints in the pillars, this work of nature ſeems to be different from any thing yet known : and it muſt be very difficult to aſſign any ſatisfactory cauſes of it.

I ſubmit to the judgment of perſons more experienced in theſe things what has occurred to me.

I ſuppoſe, for reaſons, which I ſhall give, that the ſeveral parts of theſe pillars were at firſt formed either in the ſhape of a cylinder, with the upper end in a ſpherical figure, if not both ends ; or that they were either ſpherical or oblate ſpheroids.

For, being compoſed of cryſtal of ſix ſides, and ſpar of three, and of a very fine black ſand, it may be ſuppoſed, that, as the cryſtals and ſpars united, and formed an irregular body, the fine black ſand fill'd up the interſtices, and formed ſuch cylindrical or ſpherical bodies, as yet ſoft ; but, in thin horizontal *laminæ* or plates like talc, as they moſtly appear to be ; and, if great force is applied, the ſtones will ſeparate in ſuch plates between the joints ; and thoſe parts of the pillars, which have been expoſed to the weather, and corroded by it, appear in ſuch plates. Sometimes indeed there are perpendicular joints ; as in the ſplit pillar, there ſeems to have been ſuch a one all down the pillar.

It is therefore probable, that, when this matter was in a fluid ſtate, and when the ſtratum of rock was formed, on which it was made, the fluid contiguous to the rock ſtill continued in motion ; that, after a time, ſome of the particles of matter, which

compoſe

compoſe theſe pillars, being diſengag'd from the particles of water, ceas'd to move, and form'd the parts of theſe pillars, which are next to the rock, in the above-mention'd figures; ſo much being formed only at once, or in a very ſhort time, as extends to the firſt joint: that then, either by change of ſeaſon, or ſome other accident, ſo much more water mixed with theſe particles, as prevented their continuing to form themſelves into ſuch a ſhape, and gave the former motion: that, afterwards, the decreaſe of the water might again be the cauſe of the former effect; and ſo on, till the intire pillars were formed; and the top of the laſt formed being convex, that, which was formed upon it, would probably be concave, and fit to it, either by its gravity, or by being ſofter.

All being as yet in ſome one or other of theſe figures, we ſuppoſe the gravitation of the ſecond ſtratum above the firſt joint to operate in ſuch a manner on that which was firſt formed, and ſtill ſoft, as to preſs it down; and ſo eight ſtones being round one ſtone, would naturally preſs the middle ſtone into an octagon.

The reaſons for concluding, that they were at firſt in ſome of theſe figures, are theſe:

That the concavity or convexity are either in an intire circle, or part of a circle:

That ſometimes the ends of the ſtones appear to be of a ſpherical form for ſome ſpace down, all round the ſtone; fill'd up only by a matter, that ſeparates from it, as ſhall be further explain'd.

For it is to be obſerved, that the pillar is not always ſo preſs'd out, as in each ſtone to form a regular multangular figure; but ſometimes there is a

narrow

narrow ſide, againſt which there is no ſtone, as obſerved before: ſometimes it is preſſed out only in part; and this, together with the ſpherical part, is fill'd up, probably at firſt with the floating matter; which, I ſuppoſe, when the other ſtone was formed upon it, ſo united with it, that it remains as a part of the other ſtone, and breaks off from it, when they are moved: and if this happened to the lower part of the upper ſtone, this matter, which fills up, might unite with the lower ſtone; for ſometimes this narrow ſide is ſeen in the ſame ſtone both above and below, the angle being formed in the middle of the ſtone; and then it is filled up with the matter, which united with the ſtone above, and the ſtone below.

It is to be obſerved, in purſuance of the proof, that the ſtones were originally round and ſpherical at the ends; that when the preſſure was not ſufficient to make out the angles, which I ſuppoſe to be the cauſe of theſe narrow ſides, it is in this caſe plainly ſeen, that the original circular ſhape of the ſtone is ſtill retain'd; that ſide not being horizontally in a ſtrait line, but appearing plainly to be part of a circle; as may be ſeen in the three pieces of ſtone, which I have ſent, that ſeparated from thoſe ſides, and fitted into them.

It appears alſo, that what has been preſs'd out beyond the circle at the ends is commonly flat, and not concave and convex; as it was probably made, not by the preſſure at the ends on the ſpherical part, but by the preſſure on the ſides contiguous to it; and when part of the circle is taken off, in that caſe it is probable, that the preſſure on the ſides was very great.

In one ſtone, the matter, which only in part form'd the angle, force being applied to it, came off, and

2 left

left that part ſpherical, being one of thoſe ſtones, in which one part of the ſame end is flat, and the other convex, ſwelling like a cuſhion.

This ſtone I ſent as a ſingle ſtone. It is a large octagon, twenty three inches over; but after it had been ſome time in my garden, I perceived a crack in it, and, applying force, it divided. The under ſtone had been ſo unequally preſs'd, that it is not only very thin on one ſide, but there is a large hole in it, about ſeven inches diameter, very near the edge of the ſtone; ſo that the matter muſt have been preſs'd away to the other ſide of the ſtone, not equally concave, and the ſtone above it muſt have preſs'd into the ſtone below this; in which lower ſtone the convex part, which preſs'd through the middle ſtone, muſt have been left, as it is broken: which I did not obſerve at the Cauſeway.

Some ſtones at the ſame end are partly concave, and partly convex; probably occaſion'd by ſuch an unequal preſſure; ſo that I have one, which meaſures nine inches deep on one ſide, which is convex, and four and a half on the other, which is concave: another, tho' all convex, yet is ſix inches clear at one angle, and only four at the oppoſite angle; ſo that in theſe ſtones the joint appears as indented.

We are to ſuppoſe, that, generally, the top of the lower ſtones is convex, and the bottom, conſequently, of the ſtone, that lies upon it, concave. But as ſometimes both ends of a ſtone are concave, we muſt ſuppoſe, either that the lower part of the ſtone, which ſettled on it, was harder, or, being of equal hardneſs, by its gravitation preſs'd it down.

Since I left the Causeway, I have been inform'd, that commonly, if the top of the stone in a pillar is found either concave or convex, the top likewise of every stone of that pillar is either concave or convex in the same manner; which may be a subject for future observation.

It seems probable, therefore, that all the ends were originally spherical: some of the stones, it may be, exact spheres; others, oblate spheroids; and some longer stones in a cylindrical form, and of a spherical figure, at each end. To which conjecture I have been led, by observing the shape of some I have, and of two models of two stones represented in cork by Mr. Drury, who presented the prospects of the Causeway engraved to the Royal Society, from the drawings of his sister Mrs. Susanna Drury. One of these is convex at both ends; and I have some in the same shape. This spherical figure has been altered by the pressure, in the manner I have observed: for, in the other model, part of the spherical figure is seen round the sides towards the concave end; and I have one exactly of the same kind. In those also, which I have, that are at the same time partly convex and partly concave, the convex part seems to have been the natural figure of the stone, as before described: for, where both ends are concave, that, which was probably press'd by a harder stone form'd before it, is perfectly concave; whereas that concavity, which is made by a stone probably form'd after it, is not so perfectly concave as the other; but it commonly remains convex in some part, as observed before; swelling out like a cushion press'd by any weight.

By

By all that I have been able to obſerve, the plates ſeem originally to have been horizontal: in ſome I have, which are convex, they are apparently ſo; and, as far as I could remark, in all, where the plates appear. Tho' it is probable, where the end of the end of the ſtone is concave, the laminæ or plates have in ſome meaſure been preſs'd in that form; tho' I could not certainly diſtinguiſh it in any of this kind.

Sometimes a joint near perpendicular begins as in a point from the ſide, and extends into that ſtone, and into all the ſtones of the pillar, which are beneath it; ſo as (when it has run the length of one ſtone) to take off either two ſides of the ſtones or pillar, or one ſide, and part of two ſides. This indeed ſometimes happens to be in the middle of the pillar, and in the ſame manner all the way down, ſo as to form two diſtinct pillars. Thus I have ſome, which, by this means, have a ſide leſs at one end than at the other; and I have one, in which the ſpherical part takes off at one end two ſides of the multangular figure, and makes part of a circle, as in ſome it takes off all the ſides at one end; or, more properly, the ſtone remains in its original ſpherical figure. The pieces, which fill up where the ſtone is not preſs'd into a multangular figure, ſometimes do not break off, as may be ſeen in the model.

Of the other models made by Mr. Drury, four of them fit to one another, and repreſent part of a pillar in the Cauſeway: The ſeven models not referr'd to, ſhew a variety of ſtones; the meaſures of all of them are marked in inches, and they are made by a ſcale of a tenth to an inch. The ends, which are cut ſmooth in the cork, or are marked with a pencil,

are ſuch, as he could not ſee, or neglected to obſerve.

The fourth ſtone I have ſent, which forms part of a circle, broke off from a ſtone flat at one end, is the ſpherical part of a ſtone, ſuch as it appears towards the concave end of one already mention'd.

From theſe obſervations, thoſe, who are well verſed in natural philoſophy, may poſſibly form ſome better judgment, and be more happy in their conjectures in relation to this difficult ſubject, the cauſe of the joints in the pillars of this extraordinary work of nature.

TAB. X.

Repreſents the plan and profile of the ſtones brought to Dublin by the Rev. Archdeacon Pococke.

Explanation of the Figures

Fig. 1. A plan of the pillar, with the meaſures of the lengths of the ſides, *A*, *B*, *C*, *D*.

Fig. 2. A plan of the pillar, with the lengths of the other four ſides; *viz.* *E*, *F*, *G*, *H*, and the diſtances of the circle from the ſides of the polygon.

Fig. 3. A profile of the ſtones, ſhewing the ſides *A*, *B*, *C*, *D*.

Fig. 4. A profile ſhewing the ſides *E*, *F*, *G*, *H*.

The black lines ſhew the deviation of the circles from a plane: and the large prick'd waved lines ſhew the profile of the ſwelling and concavity within them.

The upper row of figures in each ſtone ſhews the heights of the ſides at the angles ſo far as they are ſtrait.

The

Fig. 3.

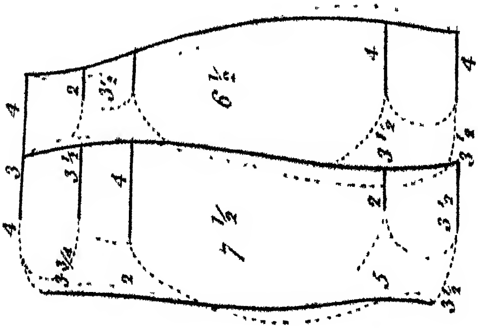


Fig. 4.

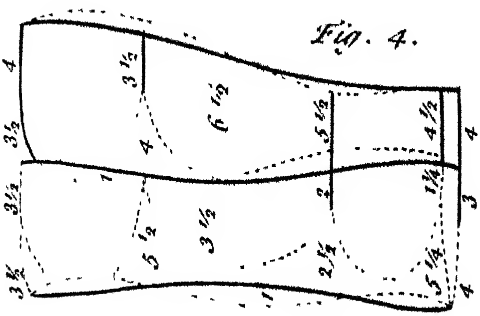
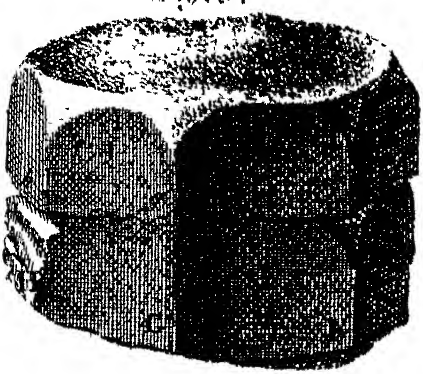


Fig. 6.



J. Mynde fc.

Fig. 1.

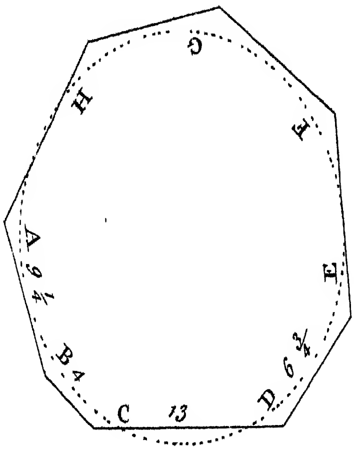


Fig. 2.

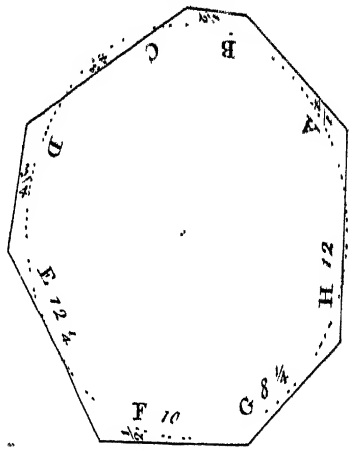


Fig. 5.

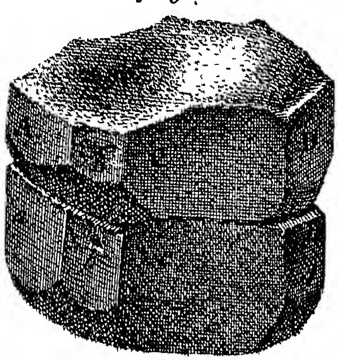
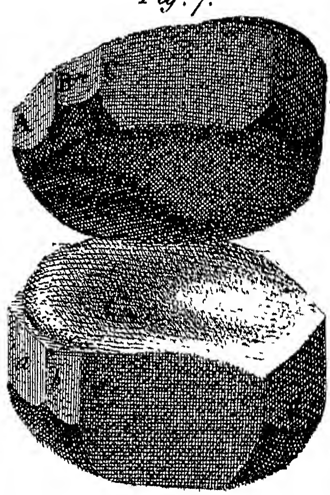


Fig. 7.



The under row of figures ſhews the remainder of the height of the ſtone at that angle; or it is the height of the angular curved pieces, which, before they fell off, were the complements filling up the priſm, and making the ſides of the pillar wholly flat, and the edges or angles of the pillar all ſtrait lines.

Fig. 5. Two upper ſtones of a pillar, as they ſtood on the Cauſeway, ſhewing 4 ſides of the pillar.

Diameter of the upper circle of the upper ſtone 22 inches.

The circle is about half an inch within the polygon at the ſide *D*; but is cut off by the ſide *C*, about three quarters of an inch.

The ſides *B* and *b* a little broken.

Fig. 6. The two ſtones turned together upſide-down; ſhewing the other four ſides of the pillar.

Diameter of the (now) uppermoſt circle about twenty-one inches.

The ſide *H* much broken. Angle *f*, rounded off from the circle to the (now) lower end of the ſtone. Angle *G* is not rounded off.

Fig. 7. The two ſtones ſeparated a little to ſhew the bottom of the upper ſtone, and top of the under one.

Diameter of the circles, which meet twenty-two inches.

The convex part of the bottom of the upper ſtone fitting the concave part of the top of the under one; and the concave part of the bottom of the upper ſtone fitting the convex part of the top of the under ſtone.

XXXV.

XXXV. *A Letter upon the ſame Subject from the Rev.* Richard Pococke, *LL.D. Archdeacon of* Dublin, *to the Rev.* Tho. Birch, *D.D. Secr. R.S.*

Dear Sir, Dublin, June 18, 1753.

Read June 28, 1753. I Received the favour of your obliging letter; and I return my hearty thanks to the Royal Society for their kind reception of the obſervations, which I ſent them on the Giant's Cauſeway. I am ſenſible, that it is difficult to make any ſuppoſitions on ſuch a ſubject, that may not be liable to ſome objections; but I ſhall be very glad, if mine put any one upon making more happy conjectures.

This day Mr. Drury brought me two maps of the Cauſeway (*ſee* Tab. XI.), one a view from the top of the cliff, and the meaſures as he pac'd; the other from my meaſures by a line; in which the letters anſwer to thoſe in the engraving after his ſiſter's drawing. I have ſent them to you, as Mr. Folkes ſome time ago deſired to know how far the Cauſeway extended into the ſea.

I have alſo ſent you four drawings by Mr. Drury, of two curious ſtones, which I brought to Dublin. I am,

Dear Sir,

Your moſt obedient humble ſervant,

Richard Pococke.

XXXVI. *A View of the Relation between the celebrated Dr.* Halley's *Tables, and the Notions of Mr.* De Buffon, *for eſtabliſhing a Rule for the probable Duration of the Life of Man; by Mr.* William Kerſſeboom, *of the* Hague. *Tranſlated from the* French, *by* James Parſons, *M. D. and F. R. S.*

—— eo diſconvenit inter
Meque et te ——

Read May 31, 1753.

§ 1. MY ſituation in life not permitting me to look over the works of the learned, was the reaſon, why I was not one of the firſt, who peruſed the *General and particular Natural Hiſtory, &c.* of Mr. de Buffon. However a little interval of leiſure allowing me to look into it, I am at a ſtand to find myſelf mention'd, in the ſame breath with the celebrated Dr. Halley and others, to receive our condemnation on account of the tables for determining the degrees of probability of the duration of human life: and as this paſſage is the occaſion of my remarks, I will begin by citing it intirely here:

" Man (ſays M. de Buffon, at the end of the
" ſecond tome) as is well known, dies at all ages;
" and altho' it may be ſaid in general, that his life
" is longer than that of almoſt any other animal, it
" cannot be denied, that it is at the ſame time more
" variable and uncertain. Attempts have been of late
" years made to know the degrees of theſe variations,

" and

" and to eſtabliſh, by obſervations, ſome certainty
" concerning the mortality of mankind of different
" ages. If theſe obſervations were ſufficiently exact,
" and a ſufficient number of them made, they would
" be of very great uſe towards knowing the number
" of the people, of their increaſe, of the conſumption
" of proviſions, of the diviſion of taxes, &c. Many
" ingenious men have ſtudied this ſubject; and lately
" Mr. Deparcieux, of the Academy of Sciences, has
" given us an excellent work, which ſerves as a rule
" for the future, with reſpect to annuities for life:
" but, as his principal view was to calculate the mor-
" tality of annuitants, and that, in general, annuitants
" for life are men in one ſtate, no concluſion can be
" drawn from it for the mortality of mankind at
" large. The tables, which he has given in the
" ſame work upon the mortality of the different re-
" ligious orders, are alſo very curious; but, being con-
" fined to a certain number of men, who live in a
" different manner from others. they are not yet ſuf-
" ficient to found exact probabilities with relation
" to the general duration of life.

" Dr. Halley, Meſſieurs Graunt, Kerſſeboom,
" Sympſon, &c. have alſo publiſhed tables of the
" mortality of mankind; and they have founded
" them upon extracts from the bills of mortality of
" ſome pariſhes of London, Breſlaw, &c. But it ap-
" pears to me, that their reſearches, however ample,
" and the reſult of long ſtudy, can afford only very
" diſtant approaches to the knowlege of the mor-
" tality of mankind in general. In order to make a
" good table of this kind, not only the regiſters of
" the pariſhes of ſuch a city as London, Paris, &c.
" ſhould

" ſhould be made uſe of, where foreigners are daily
" coming in, and natives going out, but alſo thoſe
" of the country; that, by adding together the re-
" ſults of each, the one may compenſate the other.
" M. Dupré, de St. Maur, of the French Academy,
" has begun this upon twelve country pariſhes, and
" three of thoſe of Paris: he was willing to commu-
" nicate theſe tables to me to publiſh them; and I
" the more readily do it, becauſe they are the
" only tables, upon which the probabilities of the
" life of mankind in general can be eſtabliſh'd with
" any certainty.".

As for the tables, I refer for them to the work.

§ 2. I am not at all concern'd for a defence of my table, which is ſufficient to ſupport itſelf; but I am greatly ſurpriſed, that a philoſopher ſhould condemn works, which he never either ſaw or read: for it is evident, M. de Buffon never ſaw my *Eſſays on political Arithmetic*; and that all, which he appears to know of it, is indeed very ſlightly drawn from M. Deparcieux's work, who nevertheleſs knew no more of it, as he himſelf makes it appear, than what he found in the *Bibliotheque raiſonnée* for the firſt three months of the year 1743, Tom. 30. This extract happens unluckily not to be made by an able hand; but, on the contrary, very fit, by its confuſion, and the irregularities, which run thro' it, to lead into errors. The corrections, that were made in the ſecond part of the ſame 30th Tome, are not even ſufficient to ſecure the reader from miſtakes.

Nevertheleſs M. de Buffon, without even reading the work, might have known more of it, though

written in a language, which he is very likely a ſtranger to; ſince Mr. Eames has given an excellent extract of the firſt eſſay in Engliſh, printed in N° 450 of the *Tranſactions* of the Royal Society of London, of which this philoſopher is a member; and alſo becauſe Mr. Van Rixtel has inſerted an extract of the two ſucceeding ones in N° 468. of the ſaid *Tranſactions.* Beſides which, M. de la Chapelle has given, in his 13th Tome of the *Nouvelle Bibliotheque* for the month of December in the year 1742, a very ample and judicious extract of the three eſſays together. I have alſo confirm'd my obſervations by a very intereſting and ſenſible proof, upon that part of London called the city, which is printed in the firſt three months of the year 1743, Tom. 14. of the *Nouvelle Bibliotheque*; and likewiſe in the ſecond three months of the year 1745 of the *Bibliotheque raiſonnée*, is inſerted a ſmall piece relative to my obſervations and proofs. I take the liberty of referring every reader, who may not underſtand the Low-Dutch language, to thoſe ſeveral pieces cited; and my table of the degrees of the probability of the duration of the life of man will ſupport itſelf very well againſt the haſty judgment of M. de Buffon, who certainly has too much candour not to acknowlege, after a mature deliberation of thoſe pieces mention'd, that they contain ſomething more than "very diſtant approaches to the knowlege of the "mortality of mankind in general."

§ 3. I would ſay much the ſame of that excellent piece of the learned Dr. Halley, if my ſurprize did not increaſe, the more I reflect, that this work ought to be thoroughly known to a member of the Royal Society of London; and that this very member nevertheleſs makes ſo careleſs a judgment upon

it

it. This reflection leads me to another kind of defence of that famous deceaſed author; which is to make M. de Buffon ſenſible, that "nearly the ſame "degrees of probability of the duration of the life of "man in general" are in the table of Dr. Halley, which he would have us think are in the extracts of M. Dupré's obſervations or tables, which he has publiſhed. For this purpoſe I conſtructed a table parallel to that of Dr. Halley, which begins with 1000 lives of one year old, and which I found, in the reduction of the great general numbers of Dupré's tables, to have alſo the ſmaller numbers analogous; that is, by beginning alſo with 1000 lives of a year old. Both tables are laid down as follows:

Halley's Table.			Dupré's, reduced.		
Years of Age	Number of Lives	Number of Deaths fr. Year to Yr.	Years of Age	Number of Lives	Number of Deaths fr. Year to Yr.
1	1000	145	1	1000	136
2	855	57	2	864	56
3	798	38	3	808	40
4	760	28	4	768	29
5	732	22	5	739	23
6	710	18	6	716	17
7	692	12	7	699	14
8	680	10	8	685	9
9	670	9	9	676	6
10	661	8	10	670	5
11	653	7	11	665	6
12	646	6	12	659	4
13	640	6	13	655	4
14	634	6	14	651	5
15	628	6	15	646	6
16	622	6	16	640	6

 Halley's

Halley's Table. | Dupre's, reduc'd.

Years of Age	Number of Lives	Number of Deaths fr Year to Yr	Years of Age	Number of Lives	Number of Deaths fr. Year to Yr.
17	616	6	17	634	6
18	610	6	18	628	6
19	604	6	19	622	8
20	598	6	20	614	5
21	592	6	21	609	10
22	586	7	22	599	7
23	579	6	23	592	7
24	573	6	24	585	11
25	567	7	25	574	8
26	560	7	26	566	8
27	553	7	27	558	8
28	546	7	28	550	6
29	539	8	29	544	13
30	531	8	30	531	5
31	523	8	31	526	10
32	515	8	32	516	8
33	507	8	33	508	7
34	499	9	34	501	16
35	490	9	35	485	10
36	481	9	36	475	9
37	472	9	37	466	9
38	463	9	38	457	5
39	454	9	39	452	23
40	445	9	40	429	5
41	436	9	41	424	11
42	427	9	42	413	7
43	417	10	43	406	6
44	407	10	44	400	18

Halley's Table.

Years of Age	Number of Lives	Number of Deaths fr. Year to Yr.
45	397	10
46	387	10
47	377	10
48	367	10
49	357	11
50	346	11
51	335	11
52	324	11
53	313	11
54	302	10
55	292	10
56	282	10
57	272	10
58	262	10
59	252	10
60	242	10
61	232	10
62	222	10
63	212	10
64	202	10
65	192	10
66	182	10
67	172	10
68	162	10
69	152	10
70	142	11
71	131	11
72	120	11

Dupré's, reduced.

Years of Age	Number of Lives	Number of Deaths fr. Year to Yr.
45	382	8
46	374	6
47	368	9
48	359	6
49	353	21
50	332	5
51	327	9
52	318	5
53	313	7
54	306	4
55	302	19
56	283	7
57	276	11
58	265	5
59	260	30
60	230	5
61	225	10
62	215	9
63	206	13
64	197	13
65	184	13
66	171	7
67	164	13
68	151	6
69	145	22
70	123	6
71	117	15
72	102	7

Halley's

Halley's Table.			Dupre's, reduc'd.		
Years of Age	Number of Lives	Number of Deaths fr. Year to Yr	Years of Age	Number of Lives	Number of Deaths fr. Year to Yr.
73	109	11	73	95	9
74	98	10	74	86	15
75	88	10	75	71	6
76	78	10	76	65	7
77	68	10	77	58	9
78	58	9	78	49	3
79	49	8	79	46	14
80	41	7	80	32	3
81	34	6	81	29	5
82	28	5	82	24	4
83	23	3	83	20	3
84	20		84	17	

Sum total of Dr. Halley's table . . 34000
Sum total of M. Dupré's table reduced . 33911

In the whole matter, all the difference between theſe two tables conſiſts in this, that Dr. Halley's is more perfect, more compact, and more conformable to thoſe obſervations, which conduct us all to the idea of a progreſſion nearly arithmetical, which the great number of reſearches enables us to unfold by little and little, in the repreſentation of the ſtrength of human life, when that ſtrength is become more uniform.

§ 4. If M. de Buffon will compare the table given by himſelf of the probability of the duration of life, which is founded upon that of M. Dupré de St. Maur, with

with that given by M. Deparcieux, in his ingenious work constructed upon that of Dr. Halley, he will find a like conformity between them. It is M. Deparcieux's thirteenth table, which I have before me. What follows, in Halley's column, I set down, in order to compare it with M. de Buffon's table.

Deparcieux upon Halley.			De Buffon upon Dupré of St. Maur.		
Years of Age	Lives at a Medium.		Years of Age	Duration of Life.	
	Years	M^os.		Years	Months
1	33	6	1	33	
2	38	0	2	38	
3	39	9	3	40	
4	40	9	4	41	
5	41	3	5	41	6
6			6	42	
7			7	42	3
8			8	41	6
9			9	40	10
10	40	5	10	40	2
11			11	39	6
12			12	38	9
13			13	38	1
14			14	37	5
15	37	6	15	36	9

Deparcieux upon Halley.

Years of Age	Lives at a Medium. Years	Months
16		
17		
18		
19		
20	34	2
21		
22		
23		
24		
25	30	11
26		
27		
28		
29		
30	27	11
31		
32		
33		
34		
35	25	0
36		
37		
38		

De Buffon upon Dupré of St. Maur.

Years of Age	Duration of Life. Years	Months
16	36	0
17	35	4
18	34	8
19	34	0
20	33	5
21	32	11
22	32	4
23	31	10
24	31	3
25	30	9
26	30	2
27	29	7
28	29	0
29	28	6
30	28	0
31	27	6
32	26	11
33	26	3
34	25	7
35	25	0
36	24	5
37	23	10
38	23	3

Deparcieux upon Halley.

Years of Age.	Duration of Life. Years.	Months.
39		
40	22	4
41		
42		
43		
44		
45	19	8
46		
47		
48		
49		
50	17	3
51		
52		
53		
54		
55	14	10
56		
57		
58		
59		
60	12	5

De Buffon upon Dupré of St. Maur.

Years of Age.	Duration of Life. Years.	Months.
39	22	8
40	22	1
41	21	6
42	20	11
43	20	4
44	19	9
45	19	3
46	18	9
47	18	2
48	17	8
49	17	2
50	16	7
51	16	0
52	15	6
53	15	0
54	14	6
55	14	0
56	13	5
57	12	10
58	12	3
59	11	8
60	11	1

Deparcieux upon Halley.

Years of Age.	Duration of Life. Years.	Months.
61		
62		
63		
64		
65	9	11
66		
67		
68		
69		
70	7	7
71		
72		
73		
74		
75	5	7
76		
77		
78		
79		
80	4	6
81		
82		
83		
84	3	6

De Buffon upon Dupré of St. Maur.

Years of Age.	Duration of Life. Years.	Months.
61	10	6
62	10	0
63	9	6
64	9	0
65	8	6
66	8	0
67	7	6
68	7	0
69	6	7
70	6	2
71	5	8
72	5	4
73	5	0
74	4	9
75	4	6
76	4	3
77	4	1
78	3	11
79	3	9
80	3	7
81	3	5
82	3	3
83	3	2
84	3	1

§ 5

§ 5. It is therefore ſufficiently demonſtrated, that Dr. Halley's table ought not, in M. de Buffon's opinion, to be excluded from the claſs of thoſe, which "are the only tables, upon which the probabilities "of the life of mankind in general can be eſtabliſhed "with any certainty;" far from being compriſed, in his ſevere judgment, among thoſe of authors, "whoſe "reſearches, however ample, and the reſult of long "ſtudy, can afford only diſtant approaches to the "knowlege of the mortality of mankind in general."

§ 6. M. de Buffon begins his table of the probabilities of life with a term, which precedes that of a year old, called *zero d'âge*; and from M. Dupré's obſervations, aſſigns to it a duration of 8 years. I firſt thought it an error of the preſs; but there is no room for this doubt, after what M. de Buffon ſays, "We "ſee by this table (ſays he) that one may reaſon- "ably hope, that is, lay an even wager, that an in- "fant juſt born, or who has *no age*, will live eight "years: that an infant, who is a year old, will live "thirty-three years," *&c.* This little ſpace of eight years ſtruck me; becauſe all the obſervations, which I know, are very far from it. I had therefore recourſe to the ſource, to the obſervations of Mr. Duprê himſelf, and I found it was a miſtake of M. de Buffon; the mean life of infants of *no age* being, according to M. Dupré's tables, twenty-and-five years and upward; and, from the obſervations of Juſtel, which Dr. Halley made uſe of, the mean life of a child of *no age* is above twenty-and-ſeven years.

§ 7. I might make an end here, if the ſubject did not abſolutely require my offering a word concerning the nature of both Juſtel's and M. Dupré's obſervations.

The remark has not eſcaped the ſagacity of Dr. Halley himſelf: it is, "that they want the eſſential; "which is, the number of living perſons, among "whom the obſervations upon the dead are made." If M. de Buffon had made the ſame reflections upon M. Dupré's tables, he would have found the irreparable defect of them, as well as Dr. Halley did in Juſtel's obſervations; and he would have attended more, without doubt, to the method propoſed by Monſ. Deparcieux.

§ 8. I ſhall put an end to this treatiſe by a word or two, which relates to Dr. Short's diſpleaſure againſt me. This may perhaps be unneceſſary, after the laborious and judicious extract of his book in the *Journal Britannique*, of the month of July 1750. However ſince Dr. Short joins with thoſe, who inconſiderately have accuſed me of partiality to ſuch or ſuch a city, he will have with them my defence (to ſay no more) in my letter to Mr. Eames, and in the piece already quoted in the *Nouvelle Bibliotheque*, Tom. XIV. Artic. 8. Let him read, and he will return from his prejudice: if not,

Curentur dubii medicis majoribus ægri.

Hague, May 11,
1753.

Wm. Kerſſeboom.

XXXVII. *A Letter from Father* d'Incarville, *of the Society of* Jesus, *at* Peking *in* China, *to the late* Cromwell Mortimer, *M. D. R. S Secr.*

SIR, Pekin, Nov. 15, 1751.

Read June 7, 1753

I SHOULD be glad to have it in my power to do more for your illustrious Society, both as to my situation and condition. We are so much confin'd at Peking; we have not even the liberty of going where we please by ourselves to see things; nor can we, with prudence, believe the reports of the Chinese, who make nothing of deceiving us, if they can defraud us of our money. When we can do no otherwise, we endeavour to prevent our becoming their dupes, as well as we can: and yet, notwithstanding all our precautions, we cannot answer for what intelligence we have this way, so well as for what we affirm to have seen ourselves. Every year I send to our gentlemen of the Academy at Paris what I can discover upon such matters, as I know concern them; which has given me the opportunities of sending you several curious things in natural history.

You ought to have received last year some leaves and flowers of different trees, besides a good number of seeds. The leaves and flowers of the varnish-tree, which I sent, come from the province of Nan King. This tree is different from that I saw in the king's garden at Paris. The latter is the same with what

what I ſaw at Macao; which was brought from Miſſiſſippi into France.

We have not in Europe the tree, from whoſe fruit the Toeng yeou is drawn. It were to be wiſh'd they could raiſe it there. The Toeng yeou is an oil, or natural varniſh, drawn by expreſſion from the fruits, which I have ſent you, of which they make a very great trade in China. It coſts but very little, the pound weight being worth about 7 or 8 ſols of our money. I heard ſay, that they ſell it at Paris under the name of China varniſh. It is excellent for preſerving furniture, giving them a poliſh not inferior to our varniſhes of Europe, which coſt ſo much money. Perhaps they may make ſome attempts to uſe it in Europe; but they will not ſucceed, becauſe they know not how to prepare it. This oil is ſo common in China, that the greateſt part of the people, in tolerable circumſtances, rub over their timber with it, giving it what colour they pleaſe. It not only adorns their houſes, but alſo preſerves the wood. The columns, that ſupport their houſes, and thoſe of the great room where the emperor's throne is, are varniſh'd with no other than this oil.

The Kou chou is a tree, of the bark of which they make the beſt paper in China. The common paper of their books, which looks yellowiſh, is made of particular ſpecies of Bambou, of which they prepare the young ſhoots, as we prepare hemp. They whiten it, by boiling it in lime water: in this manner they prepare the Kou chou. There is no ſilken paper in China; all the different kinds of paper here are made either of bark, hemp, or of the ſtraw of corn or rice. Sometimes they blend with this laſt the

ſtalks

ſtalks of the Typha *. The paper made of hemp or ſtraw ſer\ ..s only for wrapping up goods, or to ma e paſteboard; and that made of the bark of the cotton-plant ſerves for fans, being leſs apt to crack than any other white paper.

The white wax, produced by certain inſects, is a very curious and profitable thing. I have not yet been able to ſee any of them. What has been told me by one of our miſſionaries, who has bred them himſelf, is not ſufficient to give a proper idea of them. As to the manner of their depoſiting this wax, it appears to me, that there is ſome analogy between it, and the manner of the gum lac's being depoſited by certain ants †.

In the emperor's palace they very rarely uſe any other candles, than ſuch as are made of this wax, becauſe it never emits any ſmoke. The learned therefore uſe them only, when they compoſe an exerciſe upon their examination for degrees: for then they are confined in very ſmall rooms, where the ſmoke of tallow-candles would incommode them greatly. I believe the chief conſumption of this wax is owing to

* Typha paluſtris major of Caſpar Bauhin. Cat's-tail.

§ In order to explain this paſſage, I take the liberty of making the following remark. The Lacca-tree is the *Jujuba Indica* of the great Ray; which produces this gum. The letter-writer is miſled by what Garcias ab Horto ſays about it, that certain large-wing'd ants make this gum out of the juice ſuck'd from this tree, and depoſit it upon the *ſurculi*, &c. of the ſame: but the celebrated Ray and J. Bauhin ſay, it is exudated, and by the heat of the ſun concreted into the form, in which it is found upon the parts of this tree. There are other trees, which produce this gum, as well as this, mention'd by Hermannus.

to their coating tallow-candles with it, which I ſhall mention by-and-by. This wax is procured by boiling the matter raſped off the branches of the tree, the leaves of which are the proper nouriſhment of theſe inſects, in a large veſſel of water; the wax ſwims at the top, and, when cold, it is taken off in a cake.

The berries of the tallow-tree are of great uſe in the ſouthern provinces, where there are very few ſheep. Almoſt all the candles, ſold there, are made of the oil drawn from theſe berries. They procure this oil in the ſame manner, that I have mention'd concerning the wax; and as this oil is not of ſo good a conſiſtence as tallow, for its coheſion, when candles are made of it, they dip them in the white wax mention'd: the external coat, thus made, prevents them from guttering *. At Peking the ſame thing is done with tallow-candles; nor do I ever remember to have ſeen them run down. I imagine, that our bees-wax would anſwer the ſame purpoſes with this white wax of China.

The ſeeds of the Yen tchi come from a plant, which I think very particular; at leaſt I cannot recollect any thing like it. From theſe ſeeds or berries, when very ripe, a tincture of a fine red is drawn, as may be ſeen in the flakes of cotton charged with this colour, ſold here. They moiſten them with a little warm water, and then expreſs the colour, which is afterwards evaporated to a drineſs, and ſerves for water-colours.

The

* This is applicable to the green wax of Miſſiſſippi.

The Perſicaria, of which they make indigo in and about Peking, merits attention. Indigo is alſo made of the *Perſicaria maculata*, with which the banks of rivers and ſtreams often abound; but it is of an inferior quality to that made with the other Perſicaria, the ſeeds of which I ſent you; and this even is not of equal value with that made of the anil, ſuch as is made in the ſouthern provinces here, and in thoſe of America.

The ſtones of apricots come from a ſpecies of tree, whoſe fruit is not eatable. Theſe trees are only cultivated for theſe ſtones, from which an excellent oil is produced for burning; and which, inſtead of olive-oil, we uſe for our ſallads.

The *Hoai tze* are the cluſters of the flower of a baſtard Acacia, from whence a moſt beautiful yellow tincture is drawn, by boiling them with a little alum. The *hoang tchi tze* produces yet a finer tincture: but the fineſt yellow colour of China comes from the *hoang pe pi*; and theſe three are prepared in the ſame manner.

A kind of ſtuff is made from the cods of the wild ſilk-worm, called *kien tcheou*, excellent for wear, when made for gain, but chiefly that, which is made from ſuch cods, as I ſent you in 1749. It is ſcarce, and dear. There is another kind of *kien tcheou*, of which they ſell a large quantity at Canton: it is made of the ſilk drawn from other cods, ſome of which I ſend you this year. Theſe cods are capable of being wound on wheels or ſpindles. The firſt I ſent are only wound on ſpindles; but firſt they muſt be boiled in a ſtrong lye, made of the aſhes of the ſtalks of the *Sarazin* corn, till they are capable of

being pull'd aſunder with one's fingers, in order to turn them inſide-out, and take out the fragments of the chryſalis; and as this kind of ſtuff is work'd like other cloth, the weavers do the reſt.

The fruits of the *tong yeou*, and of the tallow-tree, which you ſhould have received laſt year, were freſher than thoſe I ſent before.

This year you will receive the cods of ſilk, which makes the ſilk called *kien tcheou*, with the butterflies, which come from them. The other things, which I ſent, want no explanation.

An Anſwer to the Queſtions upon the Natural Hiſtory *of* Foſſils.

THE empire of China abounds in mines of all ſorts, as gold, ſilver, copper, tin, lead, iron, *&c.* The provinces, which produce the greateſt quantity, are *Yun nan*, and *See tchouen*. The two greateſt rivers of China, *Kiang*, and *Hoang ho*, ſend down quantities of gold ſand. The former takes its ſource in the province of *See tchouen*, and the latter from *Coconor*: but they find mines of gold and ſilver in the provinces of *Yun nan*, *See tchouen*, *Chen ſi*, *Chan tong*, *Hou kouang*, *Fou kien*, *Kouei tcheou*, *Pe tche ſi*; but, for political reaſons, they work but few of them. I believe the principal is, leſt the greedineſs of gain ſhould excite popular inſurrections. They open them ſometimes in one place, ſometimes in another: but, upon the leaſt appearance of a riſing, they immediately ſhut them up again. We cannot give any account of what is deſired, concerning the manner of working the ſeveral mines. We are not

in

n a way of informing ourselves. I have endeavour'd or several years to procure specimens of the different mines, but could not yet obtain them. If, hereafter, I can discover any thing worth while in this matter, I shall not fail to communicate it. As to what regards petrifactions, I have only seen a few crabs, pieces of wood, and some bones, which I take to be those of buffaloes. I have sent into France specimens of all the simple drugs sold by the druggists at Peking; among which are some bits of minerals, petrified bones, *&c.* to which I expect an answer next year, and shall be better able to chuse what to send of such things, as shall be desired. This collection is one of the affairs, that cost me most trouble.

The article, that regards the deluge, makes me imagine, that the list of these things comes from the celebrated Sir Hans Sloane. I should be glad to have an opportunity of doing him pleasure, and I would do it most readily. All I know of it is this; the Chinese have but a very confused idea of an universal deluge. They only conclude from things seen upon the surface of the earth, that there must formerly have been some terrible hurricane, and that the sea had cover'd the face of the earth. A great mandarin, who had a better understanding than the Chinese commonly have, being sent into *Ho nan*, to visit several places, observed, upon the top of a very high mountain, a kind of basin, the circumference of which, formed by the mountain, was filled with different figures of fishes, shells, and marine plants, impressed upon stones: He said to another mandarin, who accompanied him, "Certainly the sea must have "been here: these fishes, shells, and plants are

"found only in the ſea." F. Gaubil ſays, the Chineſe books pretend, that ſuch impreſſions are found upon the higheſt mountains of *Thibet*, and *See tchouen.* I had an opportunity myſelf to go into the mountains about Peking, and even went up to the higheſt, but ſaw nothing of this kind; and was informed upon the ſpot, that they never found any thing like them.

The greateſt part of the cinabar of China comes from the province of *Yun nan:* and it is ſaid, there is ſome alſo in *Kiang ſi*, *Hou kouang*, and *Koui tcheou. Kang hi*, the great-grandfather of the preſent emperor, ordered a general ſearch to be made thro' the whole empire for antimony, but found none in any of the mines.

I have the honour to be, with much reſpect and eſteem,

S I R,

Peking, Nov. 15, 1751.

Your moſt humble and

obedient ſervant,

D'Incarville.

XXXVIII. *A Letter from Mr.* T. Melvil *to the Rev.* James Bradley, *D. D. F. R. S. With a Discourse concerning the Cause of the different* Refrangibility *of the* Rays *of* Light.

SIR,

Read March 1, 1753.

THO' I have not the honour of your acquaintance, I have presumed to address the inclosed paper to you, relating to optics and astronomy. The unwearied zeal which you have shewn, Sir, for the improvement of these sciences, made me conclude, that whatever has an appearance of truth or novelty on such subjects, tho' inconsiderable in itself, might not be unacceptable to you. Besides, among all the societies of the learned in Europe, I could think of no abler judge to consult on these matters, than the author of the "Aberration of Light, "and of the Variation of the Precession of the Equi-"noxes:" the two greatest discoveries, without doubt, that have been made in astronomy for half a century. Perhaps you may be able, Sir, from the many accurate observations you have already made, to decide the question relating to the velocity of light; or, at least, to determine, whether the observation proposed be practicable: for I am very sensible, that many observations, which appear easy in speculation, cannot be put in execution.

If you find any thing in the inclosed worthy of the public view, you are at liberty to lay it, in whole, or in

in part, before the Royal Society, or to make what uſe of it you pleaſe. I am,

SIR,

Geneva, Feb. 2, 1753.

Your moſt obedient humble ſervant,

T. Melvill.

Concerning the Cauſe of the different Refrangibility *of the* Rays *of* Light, *by Mr.* T. Melvill.

Read March 8, 1753.

1. IN order to account for the different refrangibility of the differently-colour'd rays, Sir Iſaac Newton †, and ſeveral of his followers, have ſuppoſed, that their particles are of different magnitudes or denſities; but if there be any analogy between the refractive power and gravity, it will produce equal velocities in all particles, whatever their magnitude or denſity be; and ſo all ſorts of rays would be equally bent from their right-lined direction.

2. It ſeems therefore a more probable opinion, which others have advanced, that the differently-colour'd rays are projected with different velocities from the luminous body; the red with the greateſt, violet with the leaſt, and the intermediate colours with intermediate degrees of velocity; for, on this hypotheſis it is manifeſt, that they will be differently refracted

† Newton's Optics, Query 29.

refracted in the prifmatic order, according to obfervation.

3. On fuppofition, that the different refrangibility of light arifes folely from the different velocities of the rays before incidence, thefe velocities muft be to one another nearly as their fines of refraction. — I fay nearly; for their exact proportion cannot be difcovered, but by the folution of the following problem, which I take this opportunity of propofing to the learned:

> If two bodies fall, in equal angles of incidence, on a fpace terminated by parallel planes, in which any power acts perpendicularly to the planes (according to the hypothefis in Prop. 94, Lib. I. of the *Principia*) the ratio of the fines of emergence to the common fine of incidence, and confequently to one another, being given, to determine the proportion of their velocities at the time of their incidence on the firft plane.

4. Their velocities in any given medium (fuppofe air) being once determined, their velocities in any other may be eafily difcover'd; for they are to thofe in air as the fine of incidence to the fine of refraction, when the ray paffes from air into the other medium †.

5. While the differently-colour'd rays are fuppofed to move with one common velocity, any pulfes, excited in the æthereal medium, muft overtake them at equal

† Principia, Phil. Nat. Prop. 95, Lib. I.

equal diſtances; and therefore the intervals of the fits of reflexion and tranſmiſſion, if they ariſe in this manner, as Sir Iſaac conjectures, would be all equal: but if the red move ſwifteſt, the violet ſloweſt, and the intermediate colours with intermediate velocities, it is plain, that the ſame pulſes muſt overtake the violet ſooneſt, the other colours in their order, and laſt of all the red; that is, the intervals of the fits muſt be leaſt in the violet, and gradually greater in the priſmatic order, agreeably to obſervation.

6. Let c denote the velocity of the æthereal pulſes, V the velocity of red light, and U that of violet; I and J the intervals of their fits, and D the diſtance between two ſucceeding pulſes: it is plain, from the nature of Newton's hypotheſis, that I is to D, as V to $C-V$: and again, D to J as $C-U$ to U: therefore, *ex æquo*, I is to J, as $CV-VU$ to $CU-VU$, from which we have the equation $C=\frac{\overline{I-J}\times VU}{I\times U-J\times V}$

Therefore, as the proportion between the intervals of the fits in red and violet, can be aſſigned by experiment, and the proportion of their velocities in any medium likewiſe, by Art. 4. the velocity of the æthereal pulſes may be eaſily computed. The velocities of the red and violet in air are nearly as 78 and 77. In the celeſtial ſpaces they are leſs, but almoſt in the ſame proportion; the intervals of their fits are by experiment as 100 and 63 †, from whence, by the canon now laid down, the velocity of the æthereal pulſes in the celeſtial ſpace is found to be to that of red

† Newton's Optics, Lib. II. Part I. Obſ. 14.

red light as 79763 to 78000. As light moves from the ſun to us, by Dr. Bradley's accurate eſtimation, in 8′ 12″ (*a*), the pulſes of the æthereal fluid muſt be propagated thro' the ſame ſpace in about 8′ 1″.

7. Hence alſo may be determined, in known meaſures, the diſtance between two ſucceeding æthereal pulſes for $D = \frac{CI - VI}{V}$.

8. Upon the hypotheſis of the different velocities of different colours, we may underſtand, at leaſt in general, the reaſon of the ſtrange analogy, diſcover'd by Sir Iſaac, between the intervals of the fits, and the ſpaces occupied by the ſeveral colours in the ſpectrum (a thing hitherto unexplained (*b*); ſince, from the velocities of the ſeveral rays, upon which depend the intervals of the fits, as has been now explained, ariſe likewiſe their ſeveral degrees of refrangibility.

9. But, as it is of great conſequence in philoſophy, to diſtinguiſh between facts and hypotheſes, however plauſible, I obſerve, that the various refrangibility, reflexibility, and inflexibility, of the different colours, and their alternate diſpoſitions, at equal intervals, to be reflected and tranſmitted, which are the whole ground-work of the Newtonian ſyſtem, are to be conſider'd as undoubted facts, deduced from experiment; but that the velocities of different rays are different in the manner now deſcrib'd, is no more than probable conjecture: and tho' this point ſhould be decided, by a method that we are now to propoſe, it

(*a*) Eames's Abridg. Tranſact. Vol. VI. p. 157.
(*b*) Compare Newt. Opt. Book I. Part 2. Prop 3. with Book II. Part 3. Prop. 16.

it would ſtill remain uncertain, whether the fits of reflexion and tranſmiſſion conſiſt in an alternate acceleration and retardation of the particles of light, or in ſomething elſe. For inſtance, it might be ſuppoſed, that every particle of light has two contrary poles, like a loadſtone; the one of which is attracted by the parts of bodies, and the other repell'd; and that, beſides their uniform rectilineal motion, the particles of differently-colour'd rays revolve in different periods round their centers; for thus their friendly and unfriendly poles being alternately turned towards the ſurfaces of bodies, they might be alternately diſpoſed to reflexion and tranſmiſſion, and that at different intervals, in proportion to the periods of their rotation. Laſtly, tho' it were proved, that the fits do proceed from an alternate acceleration and retardation of the particles of light, it would ſtill be no more than probable conjecture, that this is brought about by pulſes excited in the æthereal medium. Nay there are ſome circumſtances in theſe phænomena, that ſeem hardly intelligible by that hypotheſis alone: as, Why the intervals of the fits are leſs in denſer mediums (*a*); and, Why they increaſe ſo faſt, and in ſo intricate a proportion, according to the obliquity of incidence (*b*).

10. By Dr. Bradley's beautiful theory of the aberration of light, the ſtars appear to be removed from their true place to a certain diſtance, according to the proportion which the tranſverſe motion of the ſpectator's eye bears to the velocity of light. It is plain therefore,

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(*a*) Newt. Opt. Book II. Part 3. Prop. 17.
(*b*) Prop. 15. *ibidem*.

therefore, that, on our hypothesis, any star must have a different apparent place for every different colour; that is, its apparent disk must be drawn out by the aberration into a longitudinal form, resembling the prismatic spectrum, having its red extremity nearest its mean place. In the stars situated about the pole of the ecliptic, its length should continue always the same, tho' directed along all the different secondaries of the ecliptic in the course of a year: but in those which lie in or near the plane of the ecliptic, it should be greatest at the limits of the eastern and western aberrations, the star recovering its colour and figure, when the true and mean places coincide. But there is no hope of discovering, whether our system be true or false, by this consequence of it: for the greatest length of the dilated disk being to the whole aberration, as the difference of the velocity of the red and violet to the mean velocity of light, *i. e.* but about a 77th part of it, cannot much exceed the fourth part of a second.

11. The time which the extreme violet light takes in arriving from any distance to the eye, will be to that which the extreme red takes in coming from the same, as 78 to 77. If Jupiter be supposed in a quadrate aspect with the sun, in which position the eclipses of his satellites are most commodiously observed, his distance from the earth being nearly equal to his distance from the sun, light takes about 41′ in passing from him to the earth; therefore the last sensible violet-light, which the satellite reflects before its total immersion into Jupiter's shadow, ought to continue to affect the eye for a 77th of 41′; that is about 32″ of time after the last sensible red light is gone. It is therefore a certain consequence of our

 hypothesis

hypothefis, that a fatellite, feen from the earth, ought to change its colour about half a minute before its total immerfion, from white to a livid greenifh colour; thence into blue, and at laft evanifh in violet. I need hardly obferve, that the fame phænomenon muft take place in the time of emerfion by a contrary fucceffion of colours, beginning with red, and ending in white.

12. If this phænomenon be perceived by aftronomers, we fhall have a direct proof of the different velocities of the differently-colour'd rays, and, confequently, a mechanical account of their different degrees of refrangibility; for I fee not, to what other caufe fuch an appearance could be reafonably afcribed. If it be not, we may conclude, that rays of all colours are emitted from the luminous body with one common velocity.

Geneva, Feb. 1, 1753.

T. Melvill.

This paper was delivered to Mr. Short, in order to attend to the particular obfervation of Jupiter's fatellites recommended by Mr. Melvill, who after fome time made the following report to the Society.

EVER fince this paper of Mr. Melvill's was put into my hands, I have carefully attended the emerfions of Jupiter's firft fatellite thro' a reflecting telefcope of four feet focal length, and with a proper magnifying power; but I have not perceived the leaft alteration in the colour of the light reflected by the fatellite,

ſatellite, except in quantity. It may indeed be obſerved, that theſe emerſions are ſeen ſooner or later thro' teleſcopes of different lengths, and by eyes of different goodneſs: and it may therefore be alleged, that there is a certain quantity of time elapſed between the very firſt emerſion of the ſatellite, and the inſtant when it is perceived by the very beſt eye, aſſiſted by the beſt teleſcope; and that, during this interval, the ſucceſſion of colours, above-mention'd, is perform'd. But our author, in conſequence of his hypotheſis, ſays, that this ſucceſſion of colours may be perceived for the ſpace of 32″ after the firſt emerſion of the ſatellite; and I am fully ſatisfied, from repeated obſervations, that the quantity of time elapſed from the very firſt emerſion of the ſatellite, till it is perceived by a good eye, aſſiſted by a good teleſcope, can amount only to a very few ſeconds. So that, upon the whole, we may conclude, that it does not appear, by the obſervations of the emerſions of the firſt ſatellite of Jupiter, that the rays of different colours move with different degrees of velocity.

But our author's concluſion, that, if the rays of light emitted from Jupiter's ſatellites, at the time of their immerſion and emerſion, ſhould not be found of different colours, the rays of all colours emitted from luminous bodies will have one common velocity, ſeems only to hold good, on a ſuppoſition, that light is propagated by a continued motion, in the manner of a projectile.

Dr. Knight, in his treatiſe on attraction and repulſion (Prop. 69.) has conſider'd the propagation of light, as performed by vibrations in an elaſtic fluid, in the ſame manner as ſound is produced by

vibration

vibrations in the air: and he thinks, that it is as eafy to conceive how the velocities of the particles of light may be different, and yet take up equal times in propagating their motion from one to another through a given fpace, as to explain how founds of different tones move with equal velocities. In accounting for both, he fhews, that, in a feries of particles, which mutually repel each other, the greater their velocity, the nearer they will approach other, in communicating their motions from one to another; and confequently each of them muft move thro' a greater fpace in fo doing: wherefore the fame time may be fpent in propagating a fucceffive motion thro' a feries of particles, whofe velocity is greater, if each particle has to move thro' a greater fpace, as is fpent, where the velocity of each particle is lefs, but is continued thro' a lefs fpace. The dilemma, to which our author's reafoning feems to have reduced the doctrine of refrangibility, may therefore be confider'd as a probable argument for adopting this hypothefis of the propagation of light thro' an elaftic medium.

XXXIX. *The Cafe of the Operation for the* Empyema, *fuccefsfully performed by* Jofeph Warner, *F. R. S. and Surgeon to* Guy's Hofpital.

Read June 28, 1753.

ON the 19 of March 1752, I did myfelf the honour of communicating to the Royal Society the cafe of John Hines, on whom I had performed the operation for the empyema

empyema with ſucceſs. Since which time, I have performed the ſame operation a ſecond time with equal benefit; and therefore preſume to lay this caſe likewiſe before you, as a farther proof of its uſefulneſs under the like circumſtances.

JOHN Collier, aged 17 years, was admitted into Guy's Hoſpital on the 10 of May, 1753, on account of a complaint in his cheſt, which he had laboured under for three or four weeks. His ſymptoms were a continual pain in his left ſide, a difficulty in breathing, and an inability of lying on his right ſide, or of ſitting upright, without greatly increaſing his complaints. His pulſe was quick, and low; he had a ſhort cough, was a good deal emaciated, and appear'd ſallow in his complexion.

Upon examination, I perceived a ſmall tumor, ſituated on the anterior part of the thorax obliquely, on the left ſide of the extremity of the ſternum or breaſt-bone. There was not the leaſt diſcoloration of the integuments. On preſſing upon the tumor, his pain and difficulty of breathing were increaſed, and there appear'd ſomething like a fluctuation under my fingers. He had never any rigor, which is a ſymptom generally attending the formation of matter; but from experience I have found, that the want of this ſymptom is no proof of the contrary.

From the foregoing circumſtances, and ſymptoms, I made no doubt of the propriety of the operation, which I performed in the following manner:

The patient being properly ſituated and ſecured, I began with making an inciſion of about two inches long through the integuments, and tendinous expanſion

panſion of the oblique muſcles of the abdomen upon the moſt prominent part of the tumor: then I proceeded to the making a ſecond inciſion, of an equal length with the former, tranſverſely thro' the upper part of the *rectus* muſcle (which had a perfect healthy appearance), directing my knife forwards, betwixt the cartilaginous portions of the ſeventh and eighth ribs, into the cavity of the thorax; upon which a thick clotted matter, to the quantity of 23 ounces and upwards, was diſcharged. After the whole of the matter was diſcharged, I introduced the fore-finger of my right-hand into the cavity, with which I evidently felt the lungs quite looſe and free from adheſion, the mediaſtinum, and ſuperior part of the diaphragm; which laſt had been preſs'd ſomewhat lower than its natural ſituation by the weight of the incumbent matter. From whence it undoubtedly appear'd, that this great quantity of matter was contained in the cavity of the thorax.

After the whole of the matter was diſcharged, I introduced a linen tent, properly ſecured, into the cavity; which was continued to be introduced every day for about three weeks, now and then, as occaſion required, making uſe of the prepared ſponge-tent.

The diſcharge of matter was conſiderable for the firſt week, when it began to decreaſe gradually, till, at the end of three weeks, there was no diſcharge at all.

From this time, ſuperficial applications only were made uſe of. At the end of five weeks he was perfectly well, and has recovered his former plumpneſs, and healthy appearance.

I muſt obſerve to you, that, about two years ago, he received a violent blow on his left ſide by a fall;

fall; for which he had little or no care taken of him. He has ever ſince this accident had ſome complaints in his ſide at times, but not conſtantly; nor have they ever been ſo bad, as to prevent his acting in his buſineſs as a ſailor, till within a few weeks before he applied to me.

London, Hatton Garden,
June 28, 1753.

XL. *Extract of a Letter from Mr.* James Dodſon *to Mr.* William Mountaine, *F.R.S.*

May 26, 1753.

Read July 5, 1753.

THE world has, without diſpute, been obliged to the invention of fluxions, for many conciſe methods of calculating the peripheries, areas, and ſolidities, of curvilinear figures; but it muſt be confeſſed, at the ſame time, that the moſt uſeful, even of thoſe, had been computed before, tho' by methods more laborious; and, conſequently, ſince the truth of the principles of fluxions was long diſputed, that art ſeems rather to have received, than to have afforded, any advantage, in thoſe caſes.

Neper and Briggs calculated their ſeveral tables of logarithms, with almoſt inſuperable labour; and Van Ceulen was rendered famous for his approximation to the quadrature of the circle, on account of the acknowleged tedioufneſs of its computation. The methods of computing logarithms were indeed improved, by the aſſiſtance of the properties of the Hyperbola, and

 the

the use of infinite ſeries, before the great inventer of fluxions was known in the world: but then, the buſineſs of logarithms being purely arithmetical, the Hyperbola was foreign to the ſubject; and the nature of infinite ſeries, tho' well adapted to the purpoſe, was at that time but little underſtood.

I am ſometimes induced to believe, that, if the latter (*i. e.* the nature and uſe of infinite ſeries) had arrived at any degree of perfection, before the invention of fluxions, moſt of the ſeries, which are given for the above-named kind of calculations, and have been deduced from fluxional proceſſes, would have been diſcovered without the aſſiſtance of them: and I am of this opinion, becauſe I am certain, that many of them might have ſo been: to inſtance in both the caſes above quoted; *viz.* the ſeries, for making logarithms, and for rectifying the circle.

And firſt, the terms of one of the moſt ſimple ſeries, for expreſſing the logarithm of a given number, is compoſed of the powers of the exceſs of that number, above unity, divided by their reſpective indices; of which the firſt, third, fifth, &c. terms are affirmative, and the ſecond, fourth, ſixth, &c. terms are negative; and the difference between the ſums of the affirmative and the negative terms, is the Neperian, hyperbolic, or (as ſome call it) the natural logarithm of the given number.

Now a mathematician, who underſtands the nature and management of ſeries (altho' wholly ignorant of fluxions, or what the juſtly celebrated Dr. Halley, in his admired inveſtigation of this very ſeries, publiſhed in N° 216 of the *Philoſophical Tranſactions*, calls *ratiunculæ*, &c.) might arrive at the ſame concluſion, in the following manner:

Since

Since the logarithm of unity is univerſally determined to be nothing; that of 2, 3, 4, 10, or any other number, conſider'd as a root, is one; that of 4, 9, 16, 100, &c. conſider'd as the ſquare of that root, is two; and ſo on; it follows, that (in all caſes) the logarithm of a greater number will exceed that of its leſſer; and each logarithm will have ſome relation to the exceſs of its number, above unity, the number, whoſe logarithm is nothing: the terms of the ſeries, therefore, which will repreſent the logarithm of any number, will conſiſt of the powers of the exceſs of that number, above unity, with ſome, yet unknown, but conſtant coefficients.

That the logarithm of the ſquare of any number is twice the logarithm of its root, is a well-known property of thoſe artificial numbers; and therefore, the doubles of the particular terms of the aſſumed ſeries will conſtitute a ſeries, expreſſing the logarithm of the ſquare of the given number.

But, by the fourth propoſition of the ſecond book of Euclid, the ſquare of any quantity is equal the ſum of the ſquares of its two parts, more a double rectangle of thoſe parts; which, in this caſe (where the given number has been aſſumed, to conſiſt of unity, and an exceſs) will be unity, more twice that exceſs, more the ſquare thereof.

If, therefore, the ſeveral powers of the compound quantity (twice the exceſs of the given number above unity, more the ſquare thereof) be multiplied by the above aſſumed coefficients, and afterwards ranged under each other, according to the powers of the ſaid exceſs; their ſums will again expreſs the logarithm of the ſquare of the given number.

Now

Now, ſince the logarithm of the ſquare of the given number may be thus expreſſed by two infinite ſeries, each conſtituted of the exceſs thereof, above unity, and its powers; it follows, that the coefficients of the like powers of that exceſs, in each ſeries, will be equal between themſelves; and, conſequently, the values of the unknown coefficients may be obtained, by ſimple equations; and theſe coefficients will, by the proceſs annexed, appear to be, the reciprocals of the ſeveral indexes of the powers of that exceſs, affected alternately with the ſigns + and —, as was before found, by the quadrature of the Hyperbola, by Dr. Halley in the above-cited *Philoſophical Tranſaction*, and by many who have uſed a fluxional proceſs.

But, there is another logarithmic ſeries equally ſimple with the former, conſiſting of the ſame terms, but all affirmative. This has been demonſtrated to be the logarithm of that fraction, whoſe numerator is unity, and denominator a number, as much leſs than unity, as the former number exceeded it.

Now, if an infinite ſeries be formed from that fraction, by actual diviſion, it will conſiſt of unity, and all the powers of that defect; and if the ſeveral powers of the exceſs of this infinite ſeries above unity, be multiplied by the coefficients above-found, and ranged according to the powers of that defect, their ſums will exhibit the above-deſcribed ſeries for the logarithm of that fraction, as appears by the operation ſubjoin'd.

As to the application of theſe two ſeries, and their ſum, to the finding the logarithms of numbers; the ſame, being copiouſly treated of by Dr. Halley (in the *Philoſophical Tranſaction* before quoted) there is

no

no occaſion for the repetition thereof here. *Note*, The above tract of Dr. Halley's is printed with the explication of Sherwin's tables of logarithms (fol. 10) with many examples of the uſe thereof annexed.

Secondly, The terms of one of the beſt ſeries, for the rectification of the circle, are compoſed of the odd powers of the tangent of any arc, not exceeding 45 degrees, ſeverally divided by their reſpective indexes; of which the firſt, third, fifth, *&c.* terms are affirmative; and the ſecond, fourth, ſixth, *&c.* terms are negative; and the difference, between the ſums of the affirmative and negative terms, is the length of that arc, of which the tangent, and its powers, conſtitute the ſeries.

Now a mathematician, who underſtands the nature and management of ſeries, altho' wholly ignorant of fluxions, might inveſtigate this ſeries in the following manner:

It has been geometrically demonſtrated, that, the radius of a circle being unity, if the double of the tangent of any arc, be divided by the difference between unity, and the ſquare of that tangent, the quotient will be the tangent of twice the arc.

Now if an infinite ſeries be formed by actual diviſion, its terms will conſiſt of the doubles of the odd powers of the tangent, and will be all affirmative; which ſeries will expreſs the length of the tangent of the double of that arc, whoſe tangent and its powers conſtitute the ſame.

If a ſeries, conſiſting of the tangent and its powers, with unknown coefficients, be aſſumed (as in the former caſe) to expreſs the length of the arc; then the length of the double of that arc may be expreſſed two ways; *viz.* either by multiplying each term of

the ſeries aſſumed by the number two; or by finding the powers of the ſeries above deſcribed (which exhibits the length of the tangent of the double arc) multiplying each power by its proper coefficient, ranging the products under each other (according to the powers of the tangent of the ſingle arc) and finding their ſum.

Now, ſince the length of the double arc may be thus expreſſed, by two infinite ſeries, each conſtituted of the tangent of the ſingle arc, and its powers; therefore the coefficients of the like powers of that tangent, in each ſeries, will be equal between themſelves; and conſequently the values of the unknown coefficients may be obtained by ſimple equations.

Laſtly, ſince the ſeries, which gives the length of the tangent of the double arc conſiſts only of the odd powers of the tangent of the ſingle arc, therefore none of the even powers thereof can range therewith: now theſe will not occur in the odd powers of that ſeries; and therefore the ſeries, aſſumed to expreſs the length of the ſingle arc, whoſe double is to be compared with the ſum of the former, muſt conſiſt only of the odd powers of that tangent; and then the ſeries firſt mentioned reſults from the operation, as will appear by examining the ſame, as hereto annexed.

I am thoroughly ſenſible, that, to the learned, who are already maſters of the inveſtigation of theſe ſeries by other methods, this will appear to be an eſſay of more curioſity than uſe; but, with regard to ſtudents, I humbly conceive it will have its advantages; becauſe by thoſe, who are acquainted with the notation of algebra, and the manner of ſolving ſimple

ſimple equations, the nature of the ſeries, and the operations above uſed, may be eaſily obtained; and they may be hereby enabled to make a ſcientific uſe of logarithms in arithmetical, and of the rectification of the circle, in geometrical calculations; which at preſent cannot be done, till a much greater progreſs is made in mathematical knowlege, without great labour; as well as to examine and correct the printed tables of both ſorts, if dubious, or found to be erroneous.

The operation neceſſary to find the coefficients of a ſeries, which will expreſs the logarithm of a given number.

If the given number be repreſented by $1+n$, then the following ſeries may be aſſumed to repreſent its logarithm:

$n+xn^2+yn^3+zn^4+un^5$, &c.

And $2n+2xn^2+2yn^3+2zn^4+2un^5$, &c. will repreſent the logarithm of the ſquare of that number; *viz.* of $1+2n+nn$.

But, becauſe $2n+nn$ is the exceſs of $1+2n+nn$, above unity, therefore its logarithm will be alſo expreſſed by

$\overline{2n+nn}+x\times\overline{2n+nn}^2+y\times\overline{2n+nn}^3+z\times\overline{2n+nn}^4$, &c.

Now $\overline{2n+nn}^2=4nn+4n^3+n^4$

$\overline{2n+nn}^3= 8n^3+12n^4+6n^5$, &c.

$\overline{2n+nn}^4= 16n^4+32n^5$, &c.

$\overline{2n+nn}^5= 32n^5$, &c.

Therefore,

$$\overline{2n+nn} = 2n+nn;$$
$$x \times \overline{2n+nn}^2 = \quad 4xnn + 4xn^3 + xn^4$$
$$y \times \overline{2n+nn}^3 = \quad 8yn^3 + 12yn^4 + 6yn^5 \text{ \&c.}$$
$$z \times \overline{2n+nn}^4 = \quad 16zn^4 + 32zn^5 \text{ \&c.}$$
$$u \times \overline{2n+nn}^5 = \quad 32un^5 \text{ \&c.}$$

And the ſum of theſe is equal to the logarithm of the ſquare of $1+n$.

If an equation be formed, of the coefficients of n^2, in each of theſe expreſſions of the logarithm of that ſquare,

Then $2x = 1 + 4x$; whence $-\frac{1}{2} = x$.

And, by proceding in the ſame manner with the coefficients of, n^3, n^4, n^5, &c. and ſupplying the places of x, y, z, &c. as they ariſe, by the numbers (ſo found) we ſhall have

$2y = -\frac{4}{2} + 8y$; whence $+\frac{1}{3} = y$;
$2z = -\frac{1}{2} + \frac{12}{3} + 16z$; whence $-\frac{1}{4} = z$;
$2u = \frac{6}{3} - \frac{32}{4} + 32u$; whence $+\frac{1}{5} = u$;

Conſequently, the logarithm of $1+n$ will be expreſſed by $n - \frac{1}{2}n^2 + \frac{1}{3}n^3 - \frac{1}{4}n^4 + \frac{1}{5}n^5$, &c. as above aſſerted.

Again, ſince $\frac{1}{1-n} = 1+n+n^2+n^3+n^4+n^5$, &c. as appears by the following diviſion:

$$
\begin{array}{l}
1-n\,)\,1 \quad (1+n+n^2, \text{ \&c.} \\
\quad\quad\;\; \underline{1-n} \\
\quad\quad\quad +n \\
\quad\quad\quad \underline{+n-nn} \\
\quad\quad\quad\quad\;\; +nn
\end{array}
$$

And, ſince the exceſs of that ſeries, above unity, is the ſeries $n+n^2+n^3+n^4$, &c.

Therefore

Therefore the logarithm of $\frac{1}{1-n}$ will consist of the sums of the powers of that series, multiplied by the above-found coefficients $\frac{1}{1}, -\frac{1}{2}, +\frac{1}{3}, -\frac{1}{4}, +\frac{1}{5}$, &c.

$$\text{Now the}\left\{\begin{array}{c}2\\3\\4\\5\end{array}\right\}\begin{array}{c}\text{Power of that}\\\text{Series will be}\end{array}\left\{\begin{array}{r}n^2+2n^3+3n^4+4n^5,\ \&c.\\ n^3+3n^4+6n^5,\ \&c.\\ n^4+4n^5,\ \&c.\\ n^5,\ \&c.\end{array}\right.$$

And,

$$\left.\begin{array}{r}\frac{1}{1}\\-\frac{1}{2}\\+\frac{1}{3}\\-\frac{1}{4}\\+\frac{1}{5}\end{array}\right\}\text{of that}\left\{\begin{array}{c}1\\2\\3\\4\\5\end{array}\right\}\text{Power will be}\left\{\begin{array}{r}n+n^2+n^3+n^4+n^5,\ \&c.\\ -\frac{1}{2}n^2-\frac{2}{2}n^3-\frac{3}{2}n^4-\frac{4}{2}n^5,\ \&c.\\ +\frac{1}{3}n^3+\frac{3}{3}n^4+\frac{6}{3}n^5,\ \&c.\\ -\frac{1}{4}n^4-\frac{4}{4}n^5,\ \&c.\\ +\frac{1}{5}n^5,\ \&c.\end{array}\right.$$

The sums of which, *viz.* $\overline{n+\frac{1}{2}n^2+\frac{1}{3}n^3+\frac{1}{4}n^4+\frac{1}{5}n^5, \&c.}$ will be the logarithm of $\frac{1}{1-n}$, as above affirmed.

The operation necessary, to find the coefficients of a series, which will express the length of the arc of a circle, by the tangent of that arc, and its powers.

Let a represent the length of the arc, and t the length of the tangent thereof; then the tangent of that arc whose length is $2a$, will be $\frac{2t}{1-tt}$; which fraction is equal to the infinite series, $2t+2t^3+2t^5+2t^7+2t^9$, &c. See the division.

$$
\begin{array}{l}
1-tt\,)\,2t\qquad(\,2t+2t^3+2t^5,\ \&c.\\
\underline{\quad 2t-2t^3}\\
\qquad +2t^3\\
\qquad \underline{+2t^3-2t^5}\\
\qquad\qquad +2t^5
\end{array}
$$

And by performing the neceſſary multiplications, or diviſions, it will alſo appear, that

$$\overline{\frac{2t}{1-tt}}\Big|^{3} = 8t^3+24t^5+48t^7+80t^9,\ \&c.$$

$$\overline{\frac{2t}{1-tt}}\Big|^{5} = 32t^5+160t^7+480t^9,\ \&c.$$

$$\overline{\frac{2t}{1-tt}}\Big|^{7} = 128t^7+896t^9,\ \&c.$$

$$\overline{\frac{2t}{1-tt}}\Big|^{9} = 512t^9,\ \&c.$$

Now if we aſſume, for the value of a, the following ſeries, $t+xt^3+yt^5+zt^7+ut^9$, &c. Then $2t+2xt^3+2yt^5+2zt^7+2ut^9$, &c. $=2a$. And becauſe $\frac{2t}{1-tt}$ is the tangent of the arc, whoſe length is $2a$, therefore

$$\frac{2t}{1-tt}+x\times\overline{\frac{2t}{1-tt}}\Big|^{3}+y\times\overline{\frac{2t}{1-tt}}\Big|^{5}+z\times\overline{\frac{2t}{1-tt}}\Big|^{7}\ \&c.=2a.$$

Which expreſſion is equivalent to the ſum of the following ſeries; for

2t

$$\frac{2t}{1-tt} = 2t + 2t^3 + 2t^5 + 2t^7 + 2t^9, \ \&c.$$

$$x \times \left.\overline{\frac{2t}{1-tt}}\right|^3 = \quad 8xt^3 + 24xt^5 + 48xt^7 + 80xt^9, \ \&c.$$

$$y \times \left.\overline{\frac{2t}{1-tt}}\right| = \quad 32yt^5 + 160yt^7 + 480yt^9, \ \&c.$$

$$z \times \left.\overline{\frac{2t}{1-tt}}\right| = \quad 128zt^7 + 896zt^9, \ \&c.$$

$$u \times \left.\overline{\frac{2t}{1-tt}}\right| = \quad 512ut^9, \ \&c.$$

And (by making an equation, between $2x$, the coefficient of t^3 in the firſt-found value of $2a$, and $2+8x$, the ſum of the coefficients of t^3 in the latter) $2x = 2 + 8x$; whence $-\frac{1}{3} = x$.

And, by proceding in the ſame manner, with the coefficients of t^5, t^7, t^9, &c. and ſupplying the places of x, y, z, &c. (as they ariſe) by the numbers (ſo found) we ſhall have

$2y = 2 - \frac{24}{3} + 32y$; whence $y = +\frac{1}{5}$.

$2z = 2 - \frac{48}{3} + \frac{160}{5} + 128z$; whence $z = -\frac{1}{7}$.

And $2u = 2 - \frac{80}{3} + \frac{480}{5} - \frac{896}{7} + 512u$; whence $u = +\frac{1}{9}$.

Therefore we may conclude, that $t - \frac{1}{3}t^3 + \frac{1}{5}t^5 - \frac{1}{7}t^7 + \frac{1}{9}t^9$, &c. $= a$.

The application of this ſeries to the rectification of the circle is extant in many authors; particularly in Sherwin's tables of logarithms, above-quoted, folio 55.

When the arc is juſt 45 degrees, then $t = 1$, and the ſeries becomes $\frac{1}{1} - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9}$, &c. which converges exceedingly ſlow; but, by the aſſiſtance of a

method, given in the appendix to M. De Moivre's *Miscellanea Analytica*, it may be transformed to another, converging quicker; which method is applied to this very series, in folio 362 of the *Mathematical Repository*, Vol. I.

XLI. *A Letter from* John Lining, *M. D. of* Charles-Town, South-Carolina, *to the Rev.* Thomas Birch, *D. D. Secr. R. S. concerning the Quantity of Rain fallen there from* January 1738, *to* December 1752.

Rev. Sir, South Carolina, Charles-Town, April 9, 1753.

Read July 8, 1753.

THE favourable reception, which my former papers met with from the Royal Society, encourages me to send you a table of the quantity of rain, which fell in Charles-Town for these 15 years last past; which, if continued for half a century, might be of use, in discovering to us the changes made in a climate, by clearing the land of its woods. Tho' I formerly sent a table of the rain from 1738 to 1745 inclusive, which is publish'd in N° 487 of the *Philosophical Transactions*; yet, as I thought it would be more convenient to bring the whole into one view, I have not only added to this table the rain of those years, but have likewise distinguished the quantity which fell in the several seasons. In this table I continued the old stile to the first of last February, that the mean quantity, in each month, and in the different seasons, might be given exactly.

As

A TABLE of the Depth of Rain, in Inches and millesimal Parts, which fell in Charles-Town.

	1738	1739	1740	1741	1742	1743	1744	1745	1746	1747	1748	1749	1750	1751	1752	Greatest	Least	The Means
January	1.097	2.310	4.875	4.492	2.189	3.172	1.994	0.863	1.144	3.429	2.112	1.056	2.563	None	3.597	4.873		2.326
February	4.416	2.875	3.084	4.615	1.650	2.435	3.063	7.739	2.701	2.860	1.573	4.516	3.135	5.374	3.798	7.739	0.798	3.389
March	4.532	5.609	1.141	5.713	5.203	0.621	0.582	3.229	1.628	2.585	3.047	7.475	0.943	1.342	1.716	7.475	0.621	3.024
April	1.082	0.195	1.092	1.308	0.918	5.292	2.866	3.842	1.128	0.292	0.979	1.760	2.310	2.310	0.440	5.292	0.195	1.721
May	3.127	5.120	5.612	4.841	5.898	2.535	2.871	1.832	3.988	0.924	1.826	5.555	2.371	5.533	2.794	5.898	1.826	3.655
June	1.567	15.839	4.648	5.538	3.250	1.903	5.814	9.510	4.109	2.470	1.859	4.686	8.690	2.462	2.618	15.839	1.567	5.000
July	10.660	5.452	3.013	3.399	1.252	7.738	8.437	6.771	9.895	6 413	9.273	6.219	5.687	6.544	1.485	10.660	1.252	6.149
August	4.104	12.211	7.301	7.144	7.647	3.767	4.202	9.339	6.114	4.895	6.881	11.124	5.346	12.144	10.725	12.211	3.767	7.530
September	10.792	4.834	3.200	6.734	2.895	4.686	5.657	0.754	0.932	7.216	7.442	1.298	12.370	11.671	14.663	14.663	0.754	6.343
October	1.358	6.593	1.358	3.399	0.759	1.672	1.595	2.962	0.506	9.504	5.550	3.900	5.000	0.352	1.199	9.504	0.352	3.040
November	2.656	1.235	1.848	2.964	3.388	3.220	1.562	0 682	3.586	1.056	5.368	1.238	3.137	0.682	0.814	5.368	0.682	2.229
December	3.877	3.689	2.736	1.919	0.957	2.706	9.680	2.623	3.916	2.921	5.588	5.594	4.609	2.409	2.035	9.680	0.957	3.684
Spring	10.030	8.679	5.317	11.636	7.771	8.348	6.511	14.810	5.457	5.737	5.599	13.751	6.388	9.026	2.954	14.810	2.954	8.068
Summer	15.354	26.411	13.273	13.778	10.400	12.176	17.122	18.113	17.992	9 807	12.958	16.460	16.748	14.569	6.897	26.411	6.897	14.804
Autumn	16.254	23.638	11.759	17.277	11.301	10.125	11.454	13.055	7.552	21.615	19.873	16.322	22.716	24.167	26.587	26.587	7.552	16.913
Winter	8.843	9.797	9.076	7.072	7.517	7.920	12.105	4.449	10.931	6.089	12.012	9.355	7.744	6.688	5.456	12.105	4.449	8.340
Total Depth	49.926	65.962	65.962	52.086	36.006	39.747	48.323	50.146	39.653	44.565	51.498	54.421	56.159	50.853	42.884	65.962	36.006	48,023

The Depth of Rain in January 1753, O. S. was 2.607 inches.

Place this between p. 284, 285.

As we have many thunder-gufts in the hot months, in which a vaft quantity of rain falls, the depth of the rain, in thefe months, is thereby greatly increafed; for then we have very little rain, excepting in thunder-fhowers.

On the 30 of June, 1750, in a thunder-ftorm, there fell, in two hours, 5.335 inches of rain.

On the 16 of September, 1751, there fell, in 24 hours (but the greateft part in 6 hours) 9.955 inches of rain.

On the 15 of September, 1752, during the time of the moft violent hurricane, that was ever felt in this town, the depth of rain, which fell, was only 3.740 inches, and the greateft part of that was the fpray of the fea.

From the 17 Sept. 1751, to the 7 Auguft 1752, was the drieft feafon ever known in this province.

Since I fent an abftract of my meteorological tables to the Royal Society, I have feen Fahrenheit's thermometer in the fhade once down at the 10 degree; and once laft fummer it rofe to the 100 degree. I am,

SIR,

Your moft humble fervant,

John Lining.

A TABLE.

XLII. *A Letter from Mr.* Emanuel Mendez da Costa, *F. R. S. to the Rev.* Thomas Birch, *D. D. Secr. R. S. concerning the* Fossil, *found at* Dudley *in* Staffordshire, *and described in the* Phil. Transf. N. 496.

SIR,

Read July 12, 1753.

THE famous fossil, which Dr. Lyttelton shew'd to the Royal Society some time ago from Dudley in Staffordshire, and which is engraved and described in N° 496 of the *Transactions*, caused many arguments to what class of animals it belonged. Dr. Pococke afterwards produced two or three specimens of it extended, which proved it of the crustaceous tribe of animals. But none of his said specimens being very fair, I have taken the liberty to send you a very fair specimen of the said fossil extended, sent me last week from the iron mines at Colnbrookdale in Shropshire, and which absolutely determines me to pronounce it to be the remains of a crustaceous animal, of that kind called *Pediculi marini*, which are scaled all round, and can at will roll themselves up: And this particular kind (not to my knowlege yet discover'd from sea) may be justly synonymed *Pediculus marinus major trilobos*.

Tho' I heretofore thought it not described by any English author, yet I find it is described and figured, tho' badly, by our late member Mr. Edw. Lhuyd, in his *Lithophylacium Britannicum Ichnographia*, Epist. 1. p. 96. Table XXII. who found them in plenty

plenty in quarries, *juxta ædes nob. v. D. Gryfidii Rice de Newton, arm. prope oppidum Sancti Teilavii, in comitatu Maridunicæ.* He calls it *Buglossa curta strigosa.* He alſo gives the figure of it without any deſcription, in the *Phil. Tranſ.* N° 243.

I beg you would communicate this ſupplement to the Royal Society in my name. I remain, with great eſteem,

S I R,

London, July 12, 1753.

Your very humble ſervant,

Emanuel Mendez da Coſta.

XLIII. *Letters relating to a Theorem of Mr.* Euler, *of the* Royal Academy *of* Sciences *at* Berlin, *and F. R. S. for correcting the Aberrations in the* Object-Glaſſes *of* refracting Teleſcopes.

I.

A Letter from Mr. James Short, *F. R. S. to* Peter Daval, *Eſq; F. R. S.*

Dear Sir,

Read April 9, 1752.

THERE is publiſhed, in the Memoirs of the Royal Academy at Berlin, for the year 1747, a theorem by Mr. Euler, in which he ſhews a method of making object-glaſſes of teleſcopes, in ſuch a manner, as not to be affected by

by the aberrations ariſing from the different refrangibility of the rays of light; theſe object-glaſſes conſiſting of two *meniſcus* lens's, with water between them.

Mr. John Dollond, who is an excellent analyſt and optician, has examined the ſaid theorem, and has diſcovered a miſtake in it, which ariſes by aſſuming an hypotheſis contrary to the eſtabliſhed principles of optics; and, in conſequence of this, Mr. Dollond has ſent me the incloſed letter, which contains the diſcovery of the ſaid miſtake, and a demonſtration of it.

In order to act in the moſt candid manner with Mr. Euler, I have propoſed to Mr. Dollond to write to him, ſhewing him the miſtake, and deſiring to know his reaſons for that hypotheſis; and therefore I deſire, that this letter of Mr. Dollond's to me may be kept amongſt the Society's papers, till Mr. Euler has had a ſufficient time to anſwer Mr. Dollond's letter to him. I am,

SIR,

Surrey-ſtreet, April 9, 1752.

Your moſt humble ſervant,

James Short.

II.

A Letter from Mr. John Dollond *to* James Short, *A. M. F. R. S. concerning a Miſtake in M.* Euler*'s Theorem for correcting the Aberrations in the Object-Glaſſes of* refracting Teleſcopes.

S I R,

Read Nov. 23, 1752.

THE famous experiments of the priſm, firſt tried by Sir Iſaac Newton, ſufficiently convinced that great man, that the perfection of teleſcopes was impeded by the different refrangibility of the rays of light, and not by the ſpherical figure of the glaſſes, as the common notion had been till that time; which put the philoſopher upon grinding concave metals, in order to come at that by reflexion, which he deſpair'd of obtaining by refraction. For, that he was ſatisfied of the impoſſibility of correcting that aberration by a multiplicity of refractions, appears by his own words, in his treatiſe of Light and Colours, *Book* I. *Part* 2. *Prop.* 3. "I "found moreover, that when light goes out of air "through ſeveral contiguous mediums, as through "water and glaſs, as often as by contrary refractions "it is ſo corrected, that it emergeth in lines parallel "to thoſe in which it was incident, continues ever "after to be white. But if the emergent rays be inclined to the incident, the whiteneſs of the emerging "light will by degrees, in paſſing on from the place "of emergence, become tinged in its edges with colours."

It is therefore, Sir, ſomewhat ſtrange, that any body now-a-days ſhould attempt to do that, which

ſo long ago has been demonſtrated impoſſible. But, as ſo great a mathematician as Mr. Euler has lately publiſhed a theorem * for making object-glaſſes, that ſhould be free from the aberration ariſing from the different refrangibility of light, the ſubject deſerves a particular conſideration. I have therefore carefully examined every ſtep of his algebraic reaſoning, which I have found ſtrictly true in every part. But a certain hypotheſis in page 285. appears to be deſtitute of ſupport either from reaſon or experiment, though it be there laid down as the foundation of the whole fabrick. This gentleman puts $m:1$ for the ratio of refraction out of air into glaſs of the mean refrangible rays, and $M:1$ for that of the leaſt refrangible. Alſo for the ratio of refraction out of air into water of the mean refrangible rays he puts $n:1$, and for the leaſt refrangible $N:1$. As to the numbers, he makes $m=\frac{31}{20}$, $M=\frac{77}{50}$, and $n=\frac{4}{3}$; which ſo far anſwer well enough to experiments. But the difficulty conſiſts in finding the value of N in a true proportion to the reſt.

Here the author introduces the ſuppoſition abovemention'd; which is, that m is the ſame power of M, as n is of N; and therefore puts $n=m^a$, and $N=M^a$. Whereas, by all the experiments that have hitherto been made, the proportion will come out thus, $m-1 : n-1 :: m-M : n-N$.

The leters fixed upon by Mr. Euler to repreſent the radii of the four refracting ſurfaces of his compound object-glaſs, are f g h and k, and the diſtance of the object he expreſſes by a; then will the focal diſtance be $=\frac{1}{n\left(\frac{1}{g}-\frac{1}{h}\right)+m\left(\frac{1}{f}-\frac{1}{g}+\frac{1}{h}-\frac{1}{k}\right)-\frac{1}{a}-\frac{1}{f}+\frac{1}{k}}$. Now, ſays

* Vide Memoires of the Royal Academy of Berlin for the Year 1747.

ſays he, it is evident, that the different refrangibility of the rays would make no alteration, either in the place of the image, or in its magnitude, if it were poſſible to determine the radii of the four ſurfaces, ſo as to have $n\left(\frac{1}{g}-\frac{1}{b}\right)+m\left(\frac{1}{f}-\frac{1}{g}+\frac{1}{k}-\frac{1}{b}\right)=N\left(\frac{1}{g}-\frac{1}{b}\right)+M\left(\frac{1}{f}-\frac{1}{g}+\frac{1}{n}-\frac{1}{b}\right)$. And this, Sir, I ſhall readily grant. But when the ſurfaces are thus proportioned, the ſum of the refractions will be $=0$; that is to ſay, the emergent rays will be parallel to the incident. For, if $n\left(\frac{1}{g}-\frac{1}{b}\right)+m\left(\frac{1}{f}-\frac{1}{g}+\frac{1}{b}-\frac{1}{k}\right)=N\left(\frac{1}{g}-\frac{1}{b}\right)+M\left(\frac{1}{f}-\frac{1}{g}+\frac{1}{b}-\frac{1}{k}\right)$, then $n-N\left(\frac{1}{g}-\frac{1}{b}\right)+m-M\left(\frac{1}{f}-\frac{1}{g}+\frac{1}{b}-\frac{1}{k}\right)=0$. Alſo if $n-N:m-M::n-1:m-1$, then $n-1\left(\frac{1}{g}-\frac{1}{b}\right)+m-1\left(\frac{1}{f}-\frac{1}{g}+\frac{1}{b}-\frac{1}{k}\right)=0$; or otherwiſe $n\left(\frac{1}{g}-\frac{1}{b}\right)+m\left(\frac{1}{f}-\frac{1}{g}+\frac{1}{b}-\frac{1}{k}\right)-\frac{1}{f}+\frac{1}{k}=0$; which reduces the denominator of the fraction expreſſing the focal diſtance to $\frac{1}{a}$. Whence the focal diſtance will be $=a$; or, in other words, the image will be the object itſelf. And as, in this caſe, there will be no refraction, it will be eaſy to conceive how there ſhould be no aberration.

And now, Sir, I think I have demonſtrated, that Mr. Euler's theorem is intirely founded upon a new law of refraction of his own; but that, according to the laws diſcover'd by experiment, the aberration ariſing from the different refrangibility of light at the object-glaſs cannot be corrected by any number of refractions whatſoever. I am,

SIR,

London, March 11, 1752. Your moſt obedient humble ſervant,

John Dollond.

III.

Mr. Euler's *Letter to Mr.* James Short, *F. R. S.*

Monſieur,

Read July 8, 1753.

VOUS m'avez fait un tres ſenſible plaiſir, en ayant diſpoſé M. Dollond de remettre la propoſition de ſes objections contre mes verres objectifs, juſqu' à ce que j'y aurois repondu, et je vous en ſuis infiniment obligé. Je prend donc la liberté de vous addreſſer ma reponſe à lui, en vous priant, après l'avoir daignée de votre examen, de la vouloir bien lui remettre: et en cas que vous jugiez cette matiere digne de l'attention de la Societé Royale, je vous prierois de lui communiquer les preuves detaillées de ma theorie, que j'ai expoſée dans cette lettre. Cependant j'eſpere, que M. Dollond en ſera ſatisfait, puiſque je tombe d'accord avec lui du peu de ſucces, qu'on ſauroit ſe promettre de mes objectifs, en les travaillant ſelon la maniere ordinaire.

J'ai l'honneur d'etre, avec la plus parfaite conſideration,

Monſieur,

Votre tres humble, et

tres obéïſſant ſerviteur,

L. Euler.

Berlin, 19 Juin, 1752

IV.

IV.

A Monſieur Monſieur Dollond.

Monſieur,

Read July 8, 1753.

ETANT tres ſenſible à l'honneur que vous me faites, au ſujet des verres objectifs, que j'avois propoſé, j'ai celui de vous marquer d'abord ingenument, que j'ai rencontré auſſi ici le plus grands obſtacles dans l'execution de ce deſſein, vu qu'il s'agit de quatre faces, qui doivent etre travaillée exactement ſelon les proportions que j'avois trouvées: cependant ayant fait les experiences ſur quelqueſuns, qui parurent le mieux reuſſi, nous avons trouvé, que l'intervalle entre les deux foyers des rayons rouges et violets etoit beaucoup plus petit, qu'il ne feroit d'un verre ſimple de la meme diſtance focale. Neantmoins je dois avoüer, qu'un tel verre, quand même il bien ſeroit parfaitement executé ſur mes principes, auroit d'autres defauts, qui le mettroient au deſſous même des verres ordinaires: c'eſt qu'un tel verre n'admet qu'un tres petite ouverture en conſequence des grandes courbures, qu'on doit donner aux faces interieures: deſorte que lorſqu'on donne une ouverture ordinaire, l'image devient tres confus.

Ainſi puiſque vous vous etes donné la peine, Monſieur, d'executer de tels verres, en en faiſant des experiences *, je vous prie de bien diſtinguer les defauts, qui peuvent naitre de la diverſe refrangibilité des rayons, de ceux, qui viennent d'une trop grande ouverture: pour cet effet vous n'aurez qu'à laiſſer une tres petite ouverture.

Or

* Mr. Dollond, in his letter to Mr. Euler, here referred to, does not ſay that he had made any trials himſelf, but only he had underſtood that ſuch had been made by others, without ſucceſs.

Or ſi ma theorie etoit juſte, dont j'aurai bientot l'honneur de parler, il ſeroit moyen de remedier à ce defaut; il faudroit renoncer à la figure ſpherique qu'on donne ordinairement aux faces des verres, et tacher de leur donner une autre figure, et j'ai remarqué que la figure d'une parabole leur procureroit l'avantage, qu'ils admettroient une ouverture tres conſiderable. Notre ſavant M. Lieberkuhn s'eſt appliqué à travailler des verres dont la courbure des faces décroit depuis le milieu vers le bords, et il s'en eſt aperçu de tres grands avantages. Par ces raiſons je crois, que ma theorie ne ſouffre encore rien de ce cotè.

Pour la theorie, je conviens avec vous, monſieur, que poſant la raport de refraction d'un milieu dans un autre quelconque pour les rayons moyens comme m à 1, et pour les rayons rouges comme M à 1, la raiſon de $m - M$ à $m - 1$ ſera toujours ſi à peu prés conſtant, qu'elle ſatisfera à toutes les experiences, comme la grand Newton a remarqué. Cette raiſon ne differe non plus de ma theorie que preſque imperceptiblement: car puiſque je ſoutiens que $M = m^{\alpha}$, et que m differe ordinairement fort peu de l'unité, ſoit $m = 1 + \omega$; et puiſque $M = m^{\alpha} = 1 + \alpha\, l\, m$ à peu pres, et $l\,(1 + \omega) = l\,m = \omega$, auſſi fort à peu pres, j'aurai $m - M = 1 + \omega - 1 - a\,\omega = (1 - \alpha)\,\omega$, et $m - 1 = \omega$, donc la raiſon $\frac{m - M}{m - 1}$ ſera $= 1 - \alpha$, ou fort à peu pres conſtante. Delà je conclud, que les experiences d'ou le grand Newton a tiré ſon raport, ne ſauroient etre contraires à ma theorie.

En ſecond lieu, je conviens auſſi que ſi la raiſon $\frac{m - M}{m - 1} =$ Conſt. etoit juſte à la rigueur, il n'y auroit

plus

plus moyen de remedier au defaut qui resulte de la diverse refrangibilité des rayons, de quelque maniere qu'on disposeroit divers milieux transparens, et que l'intervalle entre les divers foyers tiendroit toujours un raport constant à la distance focale entiere du verre. Mais c'est precisement cette consideration, qui me fournit le plus fort argument: l'oeil me paroit une telle machine dioptrique parfaite, qui ne se ressent en aucune maniere de la diverse refrangibilité des rayons: quelque petite que soit sa distance focale, la sensibilité est si grande, que les divers foyers, s'il y en avoit, ne manqueroient pas de troubler tres considerablement la vision. Or il est bien certain, qu'un oeil bien constitué ne sent point l'effet de la diverse refrangibilité.

La structure merveilleux de l'oeil, et les diverses humeurs, dont il est composé, me confirme infiniment dans ce sentiment. Car s'il s'agissoit seulement de produire une representation sur le fond de l'oeil, une seule humeur auroit été suffisante; et le Createur n'y auroit pas seurement employé plusieurs. Delà je conclud, qu'il est possible d'anéantir l'effet de la diverse refrangibilité des rayons par une juste arrangement de plusieurs milieux transparens, donc puisque cela ne feroit pas possible, si la formule $\frac{m - M}{m - 1} =$ Const. étoit vraye à la rigueur, j'en tire la consequence qu'elle n'est pas parfaitement conforme à la nature.

Mais voila une preuve directe de ma these: je conçois diverse milieux transparens, *A*, *B*, *C*, *D*, *E*, *etc.* qui different entr'eux également par raport à leur densité optique: desorte que la raison de refraction de chacun dans le suivant soit le meme. Soit donc dans le passage

paſſage du premier dans le ſecond la raiſon de refraction pour les rayons rouges $= r : 1$, et pour les violets $= v : 1$; qui ſera la meme dans le paſſage du ſecond dans le troiſieme, de celuicy dans le quatrieme, du quatrieme dans le cinquieme, et ainſi de ſuite. Delà il eſt clair, que dans le paſſage du premier dans le troiſieme ſera $= r^2 : 1$ pour les rayons rouges, et $= v^2 : 1$ pour les violets: de meme dans le paſſage du premier dans le quatrieme les raiſons ſeront $r^3 : 1$ et $v^3 : 1$.

Donc ſi dans le paſſage dans un milieu quelconque la raiſon de refraction des rayons rouges eſt $= r^n : 1$, celle des rayons violets ſera $= v^n : 1$; tout cela eſt parfaitement conforme aux principes du grand Newton. Poſons $r^n = R$, et $v^n = V$, deſorte que $R : 1$, et $V : 1$ expriment les raiſons de refraction des rayons rouges et violets dans un paſſage quelconque: et ayant $n\,lr = lR$ et $n\,lv = lV$ nous aurons $lR : lr = lV : lv$, ou $\frac{lR}{lV} = \frac{lr}{lv}$. Ou bien mettes $v = r^\alpha$, et à cauſe de $lv = \alpha\, l r$, on aura $\frac{lR}{lV} = \frac{1}{\alpha}$, ou $lV = \alpha\, l R$, et partant $V = R^\alpha$.

Voilà donc le fondement du principe, que j'ai employé dans ma piece, qui me paroit encore inebranlable: cependant j'en ſoumets la deciſion à l' illuſtre Societé Royale, et à votre jugement en particulier, ayant l'honneur d'étre avec la plus parfaite conſideration, Monſieur,

Berlin, Juin 15,
1752.

Votre très humble

et très obéïſſant ſerviteur,

L. Euler.

XLIV.

XLIV. *A remarkable Case of* Fragility, Flexibility, *and* Diſſolution, *of the* Bones; *communicated by* John Pringle, *M.D. F.R.S.*

Read July 12, 1753.

MARY Hayes, of Stoke-Holy-Croſs, near Norwich in Norfolk, gave the following Account, June 21, 1752:

That ſhe was born Jan. 11, 1718, and never married, or was addicted to any kind of Intemperance: That her father was unhealthy a great part of his life, but ſhe knew not what diſeaſe he was ſubject to: That her mother died when ſhe was a child; but ſhe did not remember ſhe ever heard of her being unhealthy: That ſhe herſelf was always look'd upon as an healthy ſtrong girl, till about 15 years of age; then fell into the green-ſickneſs, and took various medicines, to no purpoſe: That this diſeaſe, as far as ſhe could recollect, was all ſhe had to complain of; doing the ordinary work in a farmer's houſe, till October 1748: Then was ſeiz'd with pain univerſally, attended with feveriſh ſymptoms. Thus ſhe continued ſome weeks; after which the pain was chiefly confined to her thighs and legs, but not increaſed by external preſſure: That, in September 1749, ſhe broke her leg, as ſhe was walking from the bed to her chair, without falling down, and heard the bones ſnap. The fracture was properly treated, and regard had to her indiſpoſition; but no callus was generated; the bones growing flexible from the knee to the ancle in a few months; as did thoſe of her other leg. Soon after, thoſe of her thighs were viſibly affected in the like manner.

 Both

Both legs and thighs then became very œdematous, and subject to excoriate, discharging a thin yellow ichor.

The winter after breaking her leg, ſhe had ſymptoms of the ſcurvy, and bled much at the gums.

Many eminent phyſicians, who were of opinion, that this diſeaſe of the bones might ariſe from acidity abounding in the blood, preſcribed for her, but without effect: unleſs the regularity of her menſtruation for the laſt eighteen months may be attributed to a chalybeate medicine: tho' medicines of that nature had no ſuch effect formerly, when ſhe was in a condition to take exerciſe, and regularly perſiſted in the uſe of them.

For ſome conſiderable time paſt ſhe had found little alteration in her complaints in general; thought her appetite and digeſtion rather better, but that the difficulty of breathing, which ſhe had long labour'd under, gradually increaſed: and the thorax appeared ſo much ſtraiten'd, as neceſſarily impeded the expanſion of the lungs. Her ſpine became much diſtorted: any motion of the vertebræ of her loins gave extreme pain; and her thighs and legs were become intirely uſeleſs; which wholly confined her to her bed, in a ſitting poſture: and the bones ſhe reſted upon, having loſt their ſolidity, were much ſpread. Alſo the ends of her fingers and thumbs, by frequent endeavours to lift herſelf up for eaſe, became very broad and flat. Then ſhe meaſured but four feet; tho', before this diſeaſe came upon her, ſhe was about five feet and a half high, and well-ſhaped.

This is the beſt information that could be obtained from her own mouth, and what was obſerved in the caſe

caſe before, and at the firſt-mention'd time, when ſhe readily conſented to the examination of her body, &c. after death.

From that time to her death, which happened Feb. 6, 1753, the chief thing ſhe complain'd of, and what the people about her obſerved, was a gradual increaſe of difficulty of breathing; a waſting of her fleſh; a ceſſation of her menſtruation for the laſt four months; a tendency in her legs to mortify, which had long been anaſarcous, and excoriated almoſt all over; ſhe retaining her ſenſes perfectly to the laſt moment of her life, and dying without ſhewing the leaſt ſigns of the agonies of death.

Two days after death, her limbs being firſt well ſtretched out, ſhe was exactly meaſured, and found wanting of her natural ſtature more than two feet two inches. Then the thorax and abdomen were opened, the ſternum being intirely removed, with part of the ribs, in order to gain at once a full view of thoſe cavities, and diſcover how the viſcera therein contained had obſtructed each other in their reſpective functions. The heart and lungs were ſound, but flaccid, and much confined in their motion; to which the enormous ſize of the liver contributed in ſome meaſure, extending quite croſs the abdomen, and bearing hard againſt the diaphragm. The lungs did not adhere to the pleura; nor was the liver ſcirrhous, but faulty only in its bulk The meſentery was ſound, except only one large ſcirrhous gland upon it. The ſpleen extremely ſmall. Nothing elſe was found obſervable in thoſe cavities.

The ſcull was not opened, to examine the brain, as intended, we wanting time; the miniſter waiting at

church for interment, and the relations growing impatient; but we had no reaſon to ſuſpect any defect there, from any previous complaint.

All her bones were more or leſs affected, and ſcarce any would reſiſt the knife: thoſe of the head, thorax, ſpine, and pelvis, nearly to the ſame degree of ſoftneſs: thoſe of the lower extremities much more diſſolved than thoſe of the upper, or of any other part. They were cut quite thro' their whole length, without turning the edge of the knife, and much leſs reſiſtance found, than firm muſcular fleſh would have made; being changed into a kind of parenchymous ſubſtance, like ſoft dark coloured liver, only meeting here and there with bony *laminæ*, thin as an egg-ſhell.

Thoſe bones were moſt diſſolved, which, in their natural ſtate, were moſt compact, and contained moſt marrow in their cavities; and the heads of them were leaſt diſſolved.

This, perhaps, is the more worthy obſervation, as it held good throughout, and looks as if the wonderful change they had undergone might be cauſed by the marrow having acquired a diſſolving quality: for it was evident the diſſolution began withinſide, from the bony *laminæ* remaining here and there on the outſide, and no-where elſe, and the pain not being increaſed at firſt by external preſſure.

The perioſteum was thicker than ordinary: the cartilages rather thinner; but no-where in a ſtate of diſſolution like the bones.

The day after this examination, ſome of the whole ſubſtance of the leg and thigh-bones, that was intirely diſſolved into a kind of pulp, was ſent to an ingenious chemiſt;

chemist; and, by the experiments which he made, he said he could discover neither acid nor alkali prevailing in it.

We, whose names are subscribed, do attest the truth of this relation.

June 25, 1753.

B. Dack, Physician.
Edward Cooper, } Surgeons.
B. Gooch, }

XLV. *Astronomical Observations made in* Surry-street, London, *by* J. Bevis, *M. D.* *and* James Short. *A. M. F. R. S.*

Read Nov. 8, 1753.

Eclipse of *Venus* by the *Moon*.

		d	h	′	″	Apparent Time.
1753	July	26	16	2	17	Venus totally hid by the Moon.
			17	5	6	Her northern cusp emerg'd: and, a few seconds after, her southern one.
				5	31	Venus was totally emerged. All these with a reflector of two feet focus. Then her diameter was found to be $32\frac{3}{4}''$, with a new kind of micrometer (of which more hereafter); and also with one of Mr. Graham's sort,

					ſort, in a two-feet Gregorian reflector.
	d	h	′	″	
July	26	21	4	18	The Moon's conſequent limb paſſed the meridian.

Eclipſe of *Mars* by the *Moon*

Aug.	20	17	6	49½	The Moon's conſequent limb paſſed the meridian.
			8	4	Mars's centre paſſed the merid. His diameter then, with both micrometers, 13¾″. The Moon's diameter 31′ 21″.
		18	6	59½	Mars totally hid by the Moon with a reflector of four feet focus. The emerſion could not be ſeen for clouds.

Occultation of β *Capricorni* by the *Moon*.

Oct.	5	7	16	50	The Moon's preceding limb paſſed the meridian.
			20	4	A ſmall ſtar, which preceded β, paſſed the meridian.
			20	19	β paſſed the meridian. Preſently after, the Moon's diameter was found to be 29′ 48″, with the new micrometer, applied to a reflector of two feet focus.
		8	21	3	The ſmall ſtar eclips'd by the Moon.
			28	48	β eclipſed by the Moon.

Oct.

	d	h	′	″	
Octob.	5	9	48	24	β emerged from the Moon. [The three laſt with a two-feet reflector.]
			48	44½	β paſſed the horary wire of the micrometer in the equatorial inſtrument of two feet focus.
			48	55	The Moon's preceding limb paſſed the ſame; the ſtar then apparently north of the Moon's ſouth limb 15′ 4″ in declination.

Eclipſe of the *Sun*.

Octob.	25	20	30	10	The eclipſe had been ſome time begun; but, for clouds, could not be ſeen till now; when the diſtance between the cuſps, meaſur'd with the new micrometer, applied to a two-feet reflector, was 12′ 26½″.
		21	15	23	The diſtance between the cuſps 29′ 49″.
			18	6	The diſtance between the viſible limbs of the Sun and Moon 11′ 32″.
		22	18	56	The diſtance between the cuſps 24′ 12″½.

The day before, about 10 in the morning, the Sun's horizontal diameter was 32′ 17″.

Theſe meaſures were all taken, when the ſun continued viſible but for a few ſeconds, through the interſtices

interſtices of flying clouds; and yet, from the nature of this micrometer, they may be very ſafely relied upon: though it would have been impoſſible to have catch'd any one of them with the common micrometer.

The principle on which this moſt excellent inſtrument is conſtructed, was laid before this Society laſt May: and it is to be hoped, that Mr. Dollond will evince the certainty of its meaſurements, from the leaſt to the greateſt angle it is capable of comprehending; and that, under every conſideration of reflexion as well as refraction by ſpherical ſurfaces; ſo as to leave no room for ſuch objections or cavils, as otherwiſe may probably be brought againſt it. For our own parts, we are fully ſatisfied of the juſtneſs thereof, from a great variety of trials and compariſons. That which we have hitherto uſed, is the firſt that has been made of the kind; and might, perhaps, have been better conſtructed in ſome reſpects, tho' in nothing material.

Applied to a reflector of only two feet, the ſcale is as large as the common micrometer can have in a forty-foot refractor; and all is done without the help of ſcrews or wires; ſo that there is no need of illuminating.

In virtue of ſuch a ſcale it is, that even fractions of ſeconds may be depended upon; as we have found, by often repeated trials on the diameters of the planets. Theſe, as well as ſmall diſtances of ſtars, may be meaſured in all directions, with equal and almoſt incredible facility, without a polar axis; as well out of doors, in a rough wind, as within.

More

More of its advantages might be mentioned, but theſe, we apprehend, may be enough to recommend it at preſent.

J. Bevis.
Ja. Short.

XLVI. *A Letter from Mr.* John Ellis *to Mr.* Peter Collinſon, *F. R. S concerning a* Cluſter-Polype, *found in the Sea near the Coaſt of* Greenland.

SIR,

Read Nov. 8, 1753.

THE marine production, that you were ſo obliging to ſend me, appears to be an animal, not a vegetable, as your friend call'd it, who ſent it to you. Upon examining it, I find it to be a ſpecies of cluſter-polype, conſiſting of many bodies united at one common baſe. This ſpecimen appears to have three-and-twenty diſtinct ones. I have ſince ſeen another, that was taken at the ſame time, that had between thirty and forty.

Each body is furniſhed at the top with eight arms or *tentaculi*, which expand themſelves in the form of a ſtar. Each arm is again furniſhed on each ſide with a row of ſmall fibres, which ſeem to do the office of fingers. In the centre of the eight arms appears the mouth, ſurrounded by ſix little ſemicircular lips ſtanding upright.

Upon diſſecting one of the bodies lengthwiſe, it appeared to conſiſt of a ſtrong muſcle, contracted

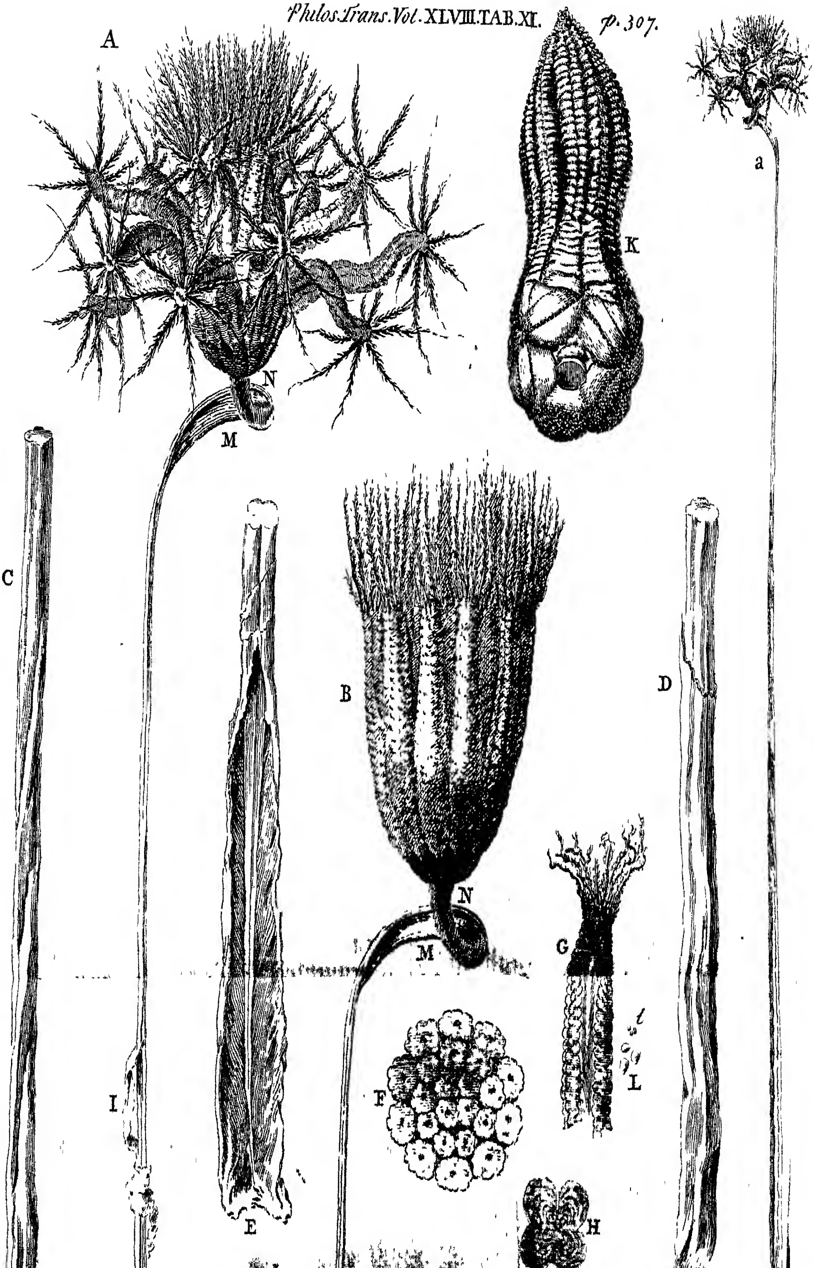
into little waves or wrinkles. In the little cavities of theſe I obſerved ſundry ſmall ſeed-like particles, perhaps the ſpawn of the animal: when magnified, they appeared of a ſpherical form, a little compreſſed.

To the centre of the baſe, where the cluſter of polypes unite, and make one body, there grows a four-ſquare bony ſtem of ſix feet long, having four grooves, one on each ſide. At the joining to the fleſhy part, the bony ſtem is very ſmall, and a little twiſted, like the turn of a ſcrew, extending a membrane like a bladder, for about two or three inches in length, and near an inch in breadth, from the fleſhy part downwards. The membrane then begins to cloſe inſenſibly, and becomes a cuticular covering to the bony ſtem, which now increaſes gradually, till it becomes a quarter of an inch ſquare. Within five or ſix inches of the bottom of the ſtem the bony part begins to grow ſmaller, till it comes to a point; and the cuticular part becomes cartilaginous, and ſupplies this tapering part with a quantity of this elaſtic ſubſtance, equal to the deficiency of the bone.

The uſe of this membrane, or bladder-like, ſkin at the top of the ſtem, may poſſibly be intended to give the animal a power to raiſe and fall itſelf in the water at pleaſure.

It appears from the twiſt in one part of the ſtem, that the ſtem, when very ſmall, and not ſo bony, had met with ſome violence, that had turned it out of its direction; the mark of which has ſtill grown on with it: for the ſtem of the other ſpecimen, taken at the ſame time, was quite even.

Upon cutting it acroſs, we diſcover the diſtinct *laminæ* to each angle, riſing from a ſmall point in the centre, and ſeparated by a croſs, that joins the oppoſite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opposite grooves. Upon putting a thin shaving of it into vinegar, a strong effervescence was immediately raised, which dissolved the gritty or coralline part, and discovered the fine membranes that inclosed it. These two substances seem to compose this bony, ivory, or coral-like stem.

The disposition of the polypes, with regard to one another, is represented by a cross section in the adjoining plate (*see* TAB. XII.); where you will observe, that 10 occupy the outward circle, 9 are in the next, and 4 are in the center.

I have learned lately from Mr. Duntze, the gentleman, who presented you with this great curiosity, the following account of it:

That it was taken in the latitude of 79 degrees north; which is within 11 degrees of the pole, and 80 English miles from the coast of Greenland, by Captain Adriaanz, commander of the Britannia, while he was on the whale-fishery last summer. The captain sounding one day in very deep water 236 fathom, two of them clung to his line. He says the arms or *tentaculi* of the polypes were of a bright yellow colour, and fully extended, when he brought them to the surface of the water; and made a most agreeable figure, like a fine full-blown flower, which the captain took them for.

I must further observe, that the *Encrinos*, or *Lilium lapideum* of the curious in fossils, so little known before, is thought to be of this class. I am,

SIR,

London, Nov. 8, 1753.

Your most obedient humble servant,

John Ellis.

References to TAB. XII.

A, The cluſter'd polype in its natural ſize, extending itſelf.

B, The ſame polype, as it was received, after it had been ſoak'd in water, and the *tentaculi* laid ſtrait.

a, The polype in miniature, with its ſtem of bone or ivory.

c, Part of the ivory ſtem twiſted.

D, The lower part of the ſtem, cover'd with a cartilage.

E, The cartilage open'd, to ſhew the tapering of the bony part.

F, The croſs ſection, to ſhew the poſition of the ſeveral bodies of the polype.

H, The croſs ſection of the bony ſtem magnified.

G, One of the bodies cut open, to ſhew its internal muſcular form.

l, The eggs or ſpawn in the natural ſize.

L, The ſame magnified.

I, The cuticular covering, which is continued from the bladder at *M* to the cartilage at *E*, or from one end of the ſtem to the other.

N, The indented muſcular baſe, where the bodies of the polype all unite.

K, A figure of the *Encrinos*, or *Lilium lapideum*, from *Roſinus*.

XLVII. *Extracts of two Letters from Father* Gaubil, *of the* Society *of* Jesus, *at* Peking *in* China, *translated from the* French.

To Dr. Cromwell Mortimer, M. D. Secretary to the Royal Society,

Peking, Nov. 2, 1752.

Read Nov. 15, 1753.

THE chronology of China, which I sent to France in 1749, had been long the object of the researches of the late M. Freret, who did me the honour to ask my sentiments upon the essays, which he sent me. This correspondence continued many years; and that learned man wrote several very curious dissertations upon that subject. I have not seen the last, which must have been published in the eighteenth tome of the Memoirs of the Academy of *Inscriptions & Belles Lettres*. I wrote to several persons detach'd pieces of what came into my thoughts on the subject, as they consulted me upon it. To avoid many inconveniences, I thought proper myself to digest my own rough draughts upon that topic; and accordingly sent the result in three parts to France in 1749.

What I have thus done in chronology, I am desirous of doing with regard to astronomy, or, at least, furnishing materials ranged in order upon that science. For it is necessary to draw up anew, or, at least, to correct, what has been printed in the work, which Mr. Costard has criticized in the *Philos. Transact.* N° 483, p. 476, 477, & *seq.* It was not sent from hence

hence to France with a view of being printed, but of being examined by men of ſkill and abilities (*a*).

I have already ſent you the catalogue of ſome antient obſervations. What relates to Jupiter is proper to correct what is remarked by Father Souciet. I have ſent either to that Father, or to M. de Liſle, a great number of corrections and additions, which are requiſite to be made; and I will ſoon digeſt into order what ſhall appear to me neceſſary. Mr. Coſtard will there, I believe, ſee the ſolution of his doubts. The Jeſuits are much obliged to him for the honour, which he has done them, in ſaying, that the beſt memoirs relating to China are owing to them. When he ſhall have examined what there is of reality with reſpect to antiquity, and the manner of coming at the knowlege of the antient hiſtory and aſtronomy, his doubts will vaniſh.

The Chineſe, without being conſummate, or even paſſable aſtronomers, might be capable of ſeeing an eclipſe, and of making obſervations on it, and of looking upon the ſhadow of the gnomon of a ſun-dial. The knowlege, which they had from time immemorial of the rectangle triangle, and of its principal properties,

(*a*) The Chineſe texts were written by men, who knew nothing, or almoſt nothing, of aſtronomy and antiquity; and there are many faults in the Chineſe contexts, and likewiſe ſome in the copies of the tranſlations, and in what was added or corrected for the impreſſion. They are only memoirs, which ought to be examined, and digeſted into a better order, after having taken the proper meaſures to reſtore the Chineſe texts, which had been alter'd, and to explain the obſcure parts by thoſe, which are intelligible; notwithſtanding which there will be many Chineſe texts ſtill unintelligible.

properties, might eaſily teach them a thouſand curious things in geometry, without knowing the theory of trigonometry.

The Chineſe, from time immemorial, knew the paſſage of the ſun in the ecliptic; they knew the ſtars; they had globes and hemiſpheres; and, by means of divers practices and precepts, received from their antients, without any great knowlege of ſpherical trigonometry, might be able upon the globe itſelf to reſolve many problems. We ought to conclude, that our antients were poſſeſſed of ſeveral kinds of knowlege, received from the patriarchs, and tranſmitted to the Chineſe. Without theſe kinds of knowlege, and theſe traditions, by mere obſervations alone, the Chineſe could not perform what they did at firſt. They never well underſtood the ſtations and retrogreſſions of the planets. Reflections upon the eclipſes of the ſun and ſtars taught them antiently, by practice, ſomething of the parallaxes of the moon.

Every thing was almoſt forgot, about the time of *Tſin chi hoam*, 240 or 246 years before Chriſt. But it is evident, that, before that time, the Chineſe muſt have known ſomething of the calculations of the eclipſes of the moon and ſun, and of ſome equations for reducing the mean motion to the true, and for calculating the ſolſtices.

Mengtſe, a claſſical author, who wrote before the burning of their books, mentions clearly enough part, at leaſt, of what I ſay. They certainly knew indifferently well the proper motion of the fix'd ſtars; which was afterwards forgot, for want of examining what was extant written in many books.

I ſhall

I ſhall ſay no more upon this ſubject; but hope, that Mr. Coſtard, and many other gentlemen in England, France, and elſewhere, will lay aſide their doubts or prejudices about the Chineſe aſtronomy and antiquities. It is, indeed, really difficult, to take the juſt medium between thoſe, who too highly extol, and thoſe who too much deſpiſe, the Chineſe literature.

I thought, that Father Hallerſtein would not have gone ſo ſoon to Macao; and when he was obliged to go thither, the plan of Peking, which I order'd to be copied for you, with ſome explications of it, was not finiſhed. I have now received it, but I cannot ſend it by the poſt with this letter; nor can I ſend it till next year, when I ſhall have a favourable opportunity for it. I ſhall then likewiſe ſend ſome other preſent for your illuſtrious Society.

On the 15 of Auguſt, an ambaſſador from the king of Portugal arrived at Macao, with preſents for the emperor of China. It were to be wiſh'd, that this embaſſy might produce ſome advantage to us; but this is extremely doubtful. The queen-mother of the king of Portugal order'd the embaſſador to deſire, that Father Hallerſtein, whom ſhe perſonally knew, might come to him to Macao, with a mandarin ſent by the emperor. The emperor conſented to this without any difficulty, and diſpatched the mandarin and Father Hallerſtein to the embaſſador. He will be here again in May.

I am of opinion, that the reigning emperor will never permit any miſſionaries in the provinces; and that they will find it very difficult to conceal themſelves. But there is no appearance, that we ſhall be

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ſent away from Peking: on the contrary, thoſe, who ſhall be ſent thither, will be well received, if they have but the qualifications requiſite.

The obſervation of Mercury in the Sun in May 1740 is very curious, and does honour to the calculation of Dr. Halley. I had ſeen that calculation; and doubting that of Manfredi in his Ephemerides from the tables of M. Caſſini, I prepared to obſerve that paſſage of Mercury here at Peking; but the weather was not favourable. And I ſee, by the obſervation made at Cambridge near Boſton in New England, that I might have had one at Peking. I hope, that we ſhall be able to obſerve the paſſage of Mercury in May next year. It is of great importance, and we ſhall endeavour to do it in the beſt manner; and if we ſucceed, we ſhall communicate it to you.

II.

To Monſ. de L'Iſle of the Royal Academy of Sciences at Paris.

SIR, Peking, Nov. 18, 1751.

YOU know, that I have, for many years, correſponded with the late famous Monſieur Freret, [Secretary of the Academy of Inſcriptions and Belles Lettres] who honoured me with his friendſhip. Monſieur de Bougainville, his ſucceſſor, wrote to me laſt year, and acquainting me with the melancholy news of his death, informed me, that he was juſt ready to commit to the preſs a great work of that eminently learned man upon the Chineſe chronology. I had ſeen ſome eſſays of it in the Memoirs of the Academy of Inſcriptions and Belles Lettres, of which

he made me a prefent. I had furnifhed him with a quantity of memoirs, as I had likewife done to others, both feculars, and thofe of our own fociety. I digefted into order all that I had collected; and, in 1749, fent a complete treatife on the Chinefe chronology, by two different ways, into France. I directed it to M. Freret, and to the Fathers of our fociety at Paris. It was in three parts. I defired them to communicate it to you, and to Monf. de Mairan. I have had no account of the arrival of that treatife, in which I had labour'd for above two-and-twenty years paft. It feem'd to me neceffary, on account of the great number of pieces, either printed or manufcript, which were fent hither on that fubject. If I find, that my treatife is loft, I can eafily digeft it into order again, from the rough draught, which I have by me.

Befides many aftronomical obfervations, which I have punctually fent you, I have tranfmitted to you the treatife of Father Duchamp upon the Indian aftronomy, a collection of antient approximations and occultations of the ftars and planets, both by each other, and by the moon, and with the moon; which I had collected and made for determining the longitude and latitude of Peking, *&c.* This year I have fent to Paris, by two different ways, a memoir, which had been defired of me, concerning the ifles of *Lequoyo*, or *Licoukicou*, which Kempfer calls *Roukou*. I have directed, that this memoir fhould be communicated to you. It is a pretty long one. I had an opportunity of being well informed about thefe ifles; but there are many things yet wanting to be known. To this memoir I have added fome remarks concerning the longitude of *Namgazaki*, and other places on the fouth

ſouth coaſt of Japan, and the ſouth coaſt of Corée, with its diſtance from Japan, and the iſland of *Touyma*, which, in the map of Father du Halde, is called *Touyla Tao*, or the iſland of *Touyla*. It ſhould be called *Touy Ma*. It is the iſle *Tsutſima*. It depends upon Japan. I have ſpoken here with ſeveral Coreans, who have been in that iſland.

I have already ſent to you obſervations made here to the cloſe of the year 1750, and during this year. I now ſend you others of 1750; and others I incloſed to you at large in 1749 and 1750. I wait for ſome anſwer from you; and eſpecially your opinion concerning the manner, in which I ought to diſpoſe my memoirs concerning the Chineſe aſtronomy. I am reſolved to put my laſt hand to that work. But memoirs of that kind ought to be examined by perſons intelligent and zealous like yourſelf.

At Peterſburg you muſt undoubtedly have ſeen what I wrote to Mr. Bayer about what the Chineſe have ſaid concerning the Huns and Turcs. Dr. Mortimer has written to me, that he had received from a nephew of Monſ. Fourmont a ſmall piece upon the origin of the Turcs and Huns, as drawn from the Chineſe books. I ſhall ſpeak again of that ſubject in the memoirs, which I have of the hiſtory of the Great Dynaſty of *Tang*. There are a great number of very intereſting things upon what the Chineſe have delivered at that time concerning the empire of the Perſians, and its deſtruction by the Mahometans; concerning the Mahometans, and the aſſiſtance, which they gave to Chineſe emperors againſt their rebels; concerning the Chriſtian religion, or the *Tatſin*, but in very obſcure terms; concerning the ſects and countries of the

Indians, Japan, Corée, Tartary, and the countries between China and the Caſpian Sea, Tybet, and its princes. All theſe particulars may be of conſiderable ſervice to unravel the eaſtern hiſtory from the year 500 of Chriſt to the year 1000 after him, and even much higher. If I have not time to reduce my memoirs into proper order, I will addreſs them to ſome learned man, like yourſelf, who can digeſt them, or ſee them digeſted.

It is ſeevral years ſince I received any thing from Father Boudier. He has, undoubtedly, ſent every thing to Paris. However, I have a good part of what he did, till 1738, and 1739; and I find, that he is much miſtaken with reſpect to the diameter of the ſun, and the obliquity of the ecliptic. I do not know whether the right aſcenſions of the ſtars are very exact. He had not then any knowlege of the aberration of the ſtars. His voyage from Chandernager to Agra, Dheli, Jaepour, is curious. I have the chart with the obſervations of the latitude and longitude of Fatepour, Agra, Dheli, Jaepour, and Chandernager. I could have wiſh'd to have had the exact diſtance from Agra and Dheli to the Ganges.

I have diſcourſed here with ſome religious Indians, who have been at Sirnegar, and as far as the ſources of the Ganges; but their account is very obſcure.

There are here a great number of Lama's and Tartars, who have gone from Laſſa, the capital of Thibet, to the lakes and mountains, where the ſources of the Ganges are, and at Latac, &c. in the country to the north of Thibet and Latac; but what they ſay is extremely confuſed; and this part of geography is ſtill very little known to us here. It would

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be of great uſe to us, if you are informed of it, and would do me the pleaſure of giving me an account of it.

Mr. Hodgſon has ſent hither, to the two German Fathers, his theory and tables of the ſatellites of Jupiter. There are a great number of obſervations made at Peking; many of which are not certain, nor proper for the forming of tables. The catalogue of the longitudes and latitudes of places is very erroneous. He will undoubtedly correct them. However, the work itſelf is a very good and uſeful one. Yourſelf and the other aſtronomers are able to judge of it much better than I can here.

The *Memoires de Trevoux* ſpeak in a manner, which, they ſay, they had learned from you, concerning the rotation of the axis of the earth, and the conſequences, which Dr. Bradley draws from it. It is a diſcovery worthy a man of his profound ſagacity; but will render it neceſſary to make a vaſt number of new calculations.

XLVIII. *A Letter of Mr.* William Shervington *to* Benjamin Franklin, *Esq*; *of* Philadelphia, *concerning the* Tranfit *of* Mercury *over the* Sun, *on the* 6 *of* May 1753, *as obferved in the Ifland of* Antigua: *Communicated by Mr.* Peter Collinfon, *F. R. S.*

S I R, Antigua, June 20, 1753.

Read Nov. 15, 1753.

MR. Benjamin Mecom having received half a dozen circulatory letters from you relating to Mercury's tranfit over the fun the 6 of laft May, he put them into my hands. One would have fufficed for our ifland, as we are not overburthen'd with men, who have a tafte that way. Hereunder I fend you the refult of my obfervation * thereof.

Sunday, May 6, at $6^h\ 7'\ 51''$, I obferved the weftern limb of Mercury to touch the weftern limb of the fun; and, at $6^h\ 10'\ 37''$, he touch'd the fame with his eaftern limb, and totally difappear'd. Lat. of the place $17^\circ\ 0'$ N. Lon. by eftimation $61^\circ\ 45'$ W. from London.

This was taken by a Graham's watch, and corrected by two altitudes taken by a moft exquifite quadrant; *viz.*

At

* Dr. Charles Rofe, who was in Antigua at this time, fays, that thefe obfervations were taken by Capt. Richard Tyrrel, of the faid ifland,

At 6^h 58′ 7″, I obſerved the diſtance of the ſun's upper limb from the zenith = 72° 21′ 30″. And, at 9^h 31′ 5″, I obſerved the ſame = 36° 17′ 0″.

By the common proceſs (which you may have, if neceſſary) I found the watch was 0° 4′ 4″ 28‴ too faſt *; therefore,

	h	′	″	‴
From — — —	6	10	37	
Take — — —	0	4	4	28
True appar^t. time of Merc^s. exit here,	6	6	32	32

Pray impart your obſervation to

Your Well-wiſher,

William Shervington.

---

iſland, and who is poſſeſſed of a valuable collection of aſtronomical inſtruments, made by Mr. Bird in the Strand, London, and that Mr. Shervington only was preſent.

* Mr. Shervington has taken the mean of theſe two altitudes from the error of his watch; and there can be no doubt that his obſervation is a good one, which, compared with that made in Surry-ſtreet by Mr. Short, *p.* 199, *l.* 1. &c. fixes the longitude of the place of his obſervation in Antigua 4^h 5′ 30″, or 61° 22′ 30″, weſt of St. Paul's, London.

XLIX. *An Account of the* Barometer, *and the State of the* Weather, *at* Dublin, *from the* 7 *of* March 1752, *to the* 28 *of* February 1753, *by* James Simon, *Esquire, F. R. S. and Secretary of the Incorporated Society at* Dublin: *Communicated by Mr.* Henry Baker, *F. R. S.*

Dear Sir, Dublin, Sept. 4, 1753.

Read Nov. 15, 1753.

I NOW ſend you my journal of the weather for the laſt year, from the 7 of March, 1752, to the 28 of February, 1753. You will, perhaps, find it too minute; but whatever fault you find in it ſhall be rectified for the future, if you are ſo kind as to let me know, how I ſhould proceed to render it uſeful. Deal with me as a friend; inform me of my miſtakes; and if you do not think it deſerves to be laid before the Royal Society, I beg you will not let it be ſeen; if otherwiſe, you have my leave to lay it before them. My chief ambition would be to deſerve their approbation and eſteem: of this you may aſſure them, and of my ſincere reſpect and regard.

I have obſerved, that the friction is ſo great in the common barometer, by the ſmallneſs of the tube, that I wrote to London for a larger tube. It is ſomething more than one quarter of an inch clear in the bore; and it took me two pounds and a quarter of clean purified mercury to fill it. The ciſtern, which receives the mercury, is pretty large, with a wide mouth; and is cover'd with a ſilk gauze, beſides the uſual

ufual wooden cover, to prevent any duft from falling into the quickfilver. In fuch a tube the friction is much lefs; and I can obferve the moft fudden change in the weather, even to an 80 part of an inch; tho' in my journal I only fet down a 40 part. For example, March 12, 1752, at night, 29 14½, is 29 inches 14½ twentieth parts and a half of an inch, or $\frac{29}{40}$ parts. I am,

Dear Sir,

Your moft humble fervant,

James Simon.

L. *A second Account of the new Method of opening the* Cornea, *for taking away the* Cataract; *by* Samuel Sharp, *Surgeon to* Guy's Hospital, *and F. R. S.*

Read Nov. 22, 1753.

I HAD the honour, last April, to lay before the Society a new method of opening the cornea, in order to take away the cataract; being intended as an improvement on Monsieur Daviel's manner of doing that operation: and having now practised it on several subjects, I here take the liberty to give a short account of the success, with a few observations on the principal phænomena attending this operation; to which I shall add a description of a farther improvement of the operation itself. For a fuller view of the history of these cases, I have here set down the ages of the patients, the dates of the days on which they underwent the operation, and the particular circumstanee of its being done on one or both eyes.

Names.	Age.	Date.	Eyes.
A B	55	April 7	1
A C	54	April 16	2
A D	70	May 12	2
A E	65	June 6	2
A F	65	July 16	2
A G	63	August 8	2
A H	48	Sept. 19	2
A I	64	Sept. 19	1
A K	67	Sept. 19	2
A L	58	Oct. 4	2
A M	47	Oct. 22	1

From

From this catalogue it appears, that the operation has been performed on nineteen eyes; and, from the moſt exact information, which I have been able to procure, the ſtate of the ſucceſs ſtands thus:

A C, *A D*, *A F*, *A G*, *A L*, all which had the operation performed on both eyes, have every one of them recovered the ſight of both eyes, to as great a perfection as can be ſuppoſed, without the help of the cryſtalline humour; that is, they can read and write, with proper ſpectacles

The firſt of them, *A C*, has found ſo much benefit, as to be able to carry on the exerciſe of his profeſſion, that of a ſurgeon.

A H ſees with both eyes, but not ſo well as the other five. I have juſt now an account from the ſurgeon, who has attended her (in a diſtant country), that her eyes look well, and her ſight improves*.

A I, another patient, at a diſtance from London, had the operation done on one eye only; which he recover'd, as my correſpondent informs me, ſo as to ſee tolerably well.

A M, on one eye only, with which he already ſees very well.

A E had it performed on both; one of which was loſt, and the other recovered; but continues inflamed, and cannot bear much light.

A B had it done on one eye only, which was loſt.

 Both

* Some weeks after this paper was read, Mr. Sharp received an account, that the pupils of both eyes had lately contracted ſo much, as hardly to leave room for the admiſſion of light; and it was apprehended the patient would ſoon become blind.

Both the eyes, in which the operation fail'd, were deſtroy'd by the ſubſequent inflammation: but, to do juſtice to this new operation, I might here take notice, with regard to the caſe of *A B*, that the ill ſucceſs was partly owing to the imperfection of my inſtrument; a diſadvantage, that muſt frequently attend on the execution of new attempts. It was the firſt operation I performed, and I had provided a knife with ſo thin a blade, that, after I had paſſed through the cornea into the anterior chamber of the eye, the point was ſo blunted, that, upon endeavouring to carry it through the cornea out on the other ſide, the blade bent, and I was apprehenſive it might break: however, withdrawing it a little, I made two or three efforts, and ſucceeded in the inciſion, and the removal of the cataract.

During this operation, the aqueous humour being diſcharged, and the patient ſtruggling, I wounded the iris; which bled profuſely, and continued for ſeveral days to diſcharge a great quantity of blood, and bloody ichor: and it is to this accident that I am inclined to impute the miſcarriage of the operation; though Mr. Daviel affirms, that the wounds of the iris have been very ſeldom followed with bad effects in his practice.

I have reſerved the mention of *A K*'s hiſtory to the laſt, becauſe of its ſingularity. She was altogether as blind as thoſe, whoſe cataracts are ripe; but hers had the appearance of a beginning cataract, being of a light blue, and but little opake. Upon making the compreſſion, the cryſtalline did not advance through the pupil, as in the other inſtances: and I found,

found, that, if I exerted more force, I ſhould ſoon evacuate all the vitreous humour. It was evident, by the great diſtance of the cataract behind the iris, that this diſappointment did not ariſe from an adheſion to the iris: however, I had immediately recourſe to the experiment of cutting through the capſula with the point of my knife; hoping, by that means, to have ſet free the cryſtalline, but it gave me no aſſiſtance. I then paſſed the *curette* (a little ſcoop) through the pupil, and turn'd it ſeveral times round, in expectation of breaking the capſula; but found not the leaſt reſiſtance to my inſtruments; ſo that both operations proved ineffectual; the circumſtances being exactly the ſame in each eye.

I have, in couching, met with cataracts of this nature; but I had no apprehenſion that I could not have diſcharged, by the wound of the cornea, the matter of a cataract, in however fluid a ſtate it might prove.

Of all the 19 there was not one, that eſcaped an inflammation; whereas, after couching, there are great numbers, who have neither inflammation nor pain. But it is to be remarked, that, notwithſtanding the violent inflammation, which ſometimes enſues after the inciſion of the cornea, even to an enlargement of the eyelids, and veſication of the *tunica conjunctiva*, the patient complains rather of a tenderneſs of the eye, upon touching it, than of pain; being generally exempt from thoſe dreadful dartings in the head, which, for the moſt part, accompany an inflammation after couching. And I believe I may aſſert, that none ſuffer'd very much in that particular, except

cept *A E*; who was extremely bad, and loſt the eye on that ſide, where the pain was.

I preſume it will not be difficult to conceive, how it ſhould happen, that theſe inflammations ſhould excite ſuch different ſymptoms, when we reflect, that, in the inciſion of the cornea, the cornea only ſuffers; and in couching, the *conjunctiva*, the *ſclerotica*, the *choroides*, and the *tunica retina*, are punctured; moſt of which organs are either tendinous or nervous; and every ſurgeon knows the painfulneſs and obſtinacy of inflammations, when they follow upon wounds and punctures of tendinous or nervous parts.

I have not mentioned, in this compariſon, the violence done to the vitreous humour; becauſe I believe it does not occaſion the ſubſequent pain; and becauſe it ſeems to be often as much or more injured in the new operation, without any notable inconvenience.

It has not occurr'd in any of theſe caſes, that the inflammation has been ſo ſlight, as to diſappear intirely in a fortnight, or three weeks; moſt of them requiring ſix weeks, and ſome longer, for the total removal of them. The firſt ten days, or more, the light is generally very offenſive; and I have obſerv'd, in three or four inſtances, that, upon forcibly opening the eyelids during that time, the patient was only ſenſible of a glare of light, though the eye then appeared clear, and he afterwards recover'd his ſight. Which I mention, to obviate the melancholy prognoſtic one would be diſpoſed to make upon a firſt examination. However, this is not to be underſtood as a conſtant fact; ſome patients diſtinguiſhing objects immediately from the time of the operation.

It

It ſometimes happens, after this operation, that the pupil loſes its circular figure: which, I imagine, is owing to the great tenderneſs of the iris, that, upon the leaſt violence, is ſubject to be ruptured; and, I ſuppoſe, in this operation, a ſlight preſſure from the back or the flat of the blade may have produced the accident in the inſtances alluded to. Poſſibly the ſudden dilatation of the pupil, from the rapid paſſage of the cataract through it, may ſometimes occaſion it; but the following hiſtory would induce one rather to aſcribe it to the cauſe, which I firſt mention'd.

Before I had thought of the knife for opening the cornea, I uſed the ſciſſars, as Monſ. Daviel directs; and, in a certain patient, after I had made the wound of the cornea, and was going to compreſs the eye, for the expulſion of the cataract, I diſcover'd, that, from the diſturbance I had given to the humours by the foregoing proceſs, it was ſunk almoſt as much as if it had depreſſed by a couching needle. I therefore left it in that ſituation, and the man afterwards ſaw very well; tho' the cataract remained viſible ſomething below the pupil.

Now, in this inſtance, the cataract had not paſſed thro' the pupil; and yet it was lacerated, ſo as to loſe its circular form: but, whatever may be the cauſe, I do not find, that the accident itſelf proves prejudicial to the ſight.

I ſhall finiſh theſe obſervations with a remark on one diſadvantage, to which this operation and couching are incident; and that is a poſſibility of an incomplete *gutta ſerena* being complicated with the cataract. It has, indeed, been a rule with ſurgeons

tho' the cataract appear fair, never to perform the operation, but when the patient is ſenſible of a glimmering of light; becauſe a total privation of ſight argues ſome other diſorder of the eye. But the criterion is not infallible; for it may happen, that a *gutta ſerena* ſhall prevail to a degree, that does not take away the ſenſe of light, and yet prevents the diſtinction of objects: So that when a cataract is thus circumſtanced, the operation will be fruitleſs.

It remains now to ſpeak of the operation itſelf. In my former paper, after having deſcribed the manner of making the inciſion, I directed the operator to compreſs the inferior part of the globe of the eye with his thumb gently, till the cataract ſhould be expell'd through the inciſion of the cornea, upon the patient's cheek; and in this method I have perform'd it upon ſeveral ſubjects. But remarking, that tho', upon the evacuation of the aqueous humour, the cryſtalline readily advanced through the pupil into the anterior chamber, yet that it required ſome force to expel it from its membrane through the wound of the cornea, and in that action it ſometimes ſuddenly drew after it a portion of the vitreous humour, I changed my method, and no longer preſs'd the eye, when once the cryſtalline was in the anterior chamber, but immediately ſtuck the point of my knife into the body of it, and extracted it contained in its capſula, without ſpilling any of the vitreous humour.

This new proceſs, I ſuppoſe, will be found of conſiderable advantage, as it will, in a great meaſure, remove the danger of evacuating the whole, or too much, of the vitreous humour: though it may be obſerved, to the praiſe of this operation, that, contrary

trary to expectation, a large quantity of this humour (perhaps a third part, or more) has been ſometimes diſcharged, without any bad conſequence.

I have ſuppoſed, that the great and ſole benefit ariſing from this improvement, is the eaſy ſeparation of the cryſtalline from the bed of the vitreous humour, ſo that none of the vitreous humour ſhall be evacuated. But perhaps it will alſo be approved of, as it will render unneceſſary the meaſure preſcribed by Monſ. Daviel, of wounding the membrane of the cryſtalline, before we proceed to the extraction of the cryſtalline itſelf: To which purpoſe he adviſes the flap of the cornea to be ſuſpended with a ſmall ſpatula; then, with a pointed cutting needle, to wound the ſurface of the cryſtalline; after which, to introduce the ſame ſpatula through the pupil, in order to detach the cataract from the iris, and then proceed to the expulſion.

I have here recited theſe proceſſes of M. Daviel's operation, which are calculated merely to procure an eaſy operation of the cryſtalline from the vitreous humour: But they are difficult to the operator, fatiguing to the patient, and, I ſhould hope, altogether needleſs, if the knife be uſed in the manner which I have recommended: For, whether the capſula of the cryſtalline be nothing more than the duplicature of the membrane of the vitreous humour, or whether it be a proper coat, which is alſo cover'd by the membrane of the vitreous humour; in either caſe, ſince by compreſſion the chryſtalline advances with ſo much facility through the pupil, it will be eaſily ſeized by the knife, and removed from the vitreous humour, with its inveloping membrane:

Whereas, in making an incifion on the furface of the cryftalline, and wounding its capfula, the cryftalline will frequently flip out of the capfula, which will be left behind: And, in fact, this has happen'd to M. Daviel, who advifes pincers, and other inftruments, in order to extract the remaining membrane. However, I fhall here obferve, in regard to the capfula of the cryftalline, that, fhould the humour flip out of it, before it be feized by the knife, it poffibly will wafte; for, in milky cataracts, when the fluid is difcharged, the membrane, in length of time, waftes: Whole cataracts, with the inveloping membrane likewife, fometimes wafte: And in one of my patients (*A. C.*) the cryftalline, from the mere preffure in the operation, burft out of its capfula, which I left in the eye; but in fome weeks it intirely wafted. However, if the removing of the capfula fhould, by future experience, be found neceffary, it may be conveniently done by the *curette*; one of the inftruments M. Daviel recommends upon that occafion. This inftrument may be alfo ufed for the extraction of a cataract, which has been broken to pieces by the couching needle in a former operation, and for the removal of the capfula of bag-cataract, when the fluid only has been difcharged, and the bag remains behind; but it will be moft eminently ufeful in detaching the cryftalline from the back part of the iris, when any portion of it happened to adhere: Which circumftance would render the operation fruitlefs, without fuch a precaution.

It has not happened, in any of the cafes I have treated, that, either during the operation, or after the operation, the iris has been pufh'd forwards, or

insinuated itself through the wound of the cornea, forming a *staphyloma*; but M. Daviel speaks of it as an occurrence he has met with, and says it may be easily replaced by the small spatula.

I presume a greater number of operations will prove this account very deficient: But I have here communicated all that I have done, and all that I know on the subject; not having suppress'd one experiment, nor, to the best of my remembrance, one circumstance, either to the honour or disgrace of the operation.

It is to be hoped, that, when it shall be more generally practised, ingenious men will render it still more perfect: And I should not be surprised, if the use of a *speculum oculi* should hereafter be esteemed an improvement: But then it must be contrived so, as that it shall not compress the globe of the eye; or, if it does, the operator must be careful to remove it in the instant the incision is making, lest, by continuing the pressure after the wound is made, all the humours should suddenly gush out.

It was impossible for me to make any remarks on this operation, without criticising on M. Daviel, as M. Daviel has not only the honour of having first practised it, but is the only writer who has treated the subject, at least that I am acquainted with. I therefore flatter myself, that this attempt to improve upon what he has laid down, will not be construed as a reflection on him, or his practice: For, however his invention may be perfected by others, in my opinion, it is still to him principally, that the world will be indebted for the benefit of the discovery.

LI. *An attempt to explain an antient Roman inſcription, cut upon a ſtone lately found at* Bath. *By* John Ward, LL.D. *Rhet. Prof. Greſh. and V. P. R. S.*

Read Nov. 22, 1753.

A COPY of this inſcription was firſt communicated to this Society by Mr. *Henry Baker*, who received it in a letter from *John Browning* Eſq; dated the fourth of July laſt. And the Society being then pleaſed to refer it to my further conſideration, ſeveral other copies of it were in a few days ſent me by my freinds from *Bath*, and other places. But upon comparing them one with another, I found they all differed in ſome reſpects; excepting two only, one of which had been copied from the other. And therefore, as I could not by this means ſettle the reading to my ſatisfaction, Dr. *Richard Davies* the phyſician being then in *London*, I acquainted him with the affair; who was ſo obliging, as to promiſe me, that upon his return to *Bath* he would procure for me an exact copy from his own inſpection. And accordingly he applied himſelf for that purpoſe to the ingenious ſtatuary at *Bath*, Mr. *Prince Hoare*; who apprehending that a caſt of it in plaiſter of *Paris* might expreſs both the form of the letters, and the defects of them, in their preſent ſtate, with greater exactneſs than a writen copy, was ſo kind as to take one off from the ſtone; which I received afterwards from Dr. *Davies*.

The ſtone was diſcovered upon the twenty-ſecond of June laſt, about five feet under ground, in digging the cellar of a houſe, which was rebuilding at the

lower end of *Stall Street.* Among the rubbiſh of the old houſe, when it was pulled down, was a large quantity of walling ſtone, which had on it the marks of fire; ſo that probably ſome building had formerly ſtood there, which was burnt. And in ſinking the ground about four or five feet lower than the ſtone, they found, as I am informed, two coins of the emperor *Carauſius*, in baſe metal, and very much defaced; tho upon one of them the following inſcription was legible: IMP CARAVSIVS PF. In July 1727 the beautiful gilt head, which is now preſerved in the town houſe, was dug up at the other end of this ſtreet, not far from the *King's bath*, about ſixteen feet below the ſurface of the earth, as they were making a common ſhore through the town (1).

The ſtone, upon which this inſcription is cut, has been generally taken for a pedeſtal, either of a ſtatue, or ſome other ſolid body, which it once ſupported. Tho from the appearance of the horizontal plane at the top Mr. *Hoare* is of the opinion, that nothing was formerly placed upon it; and ſuppoſes, that the ſinking in the middle, with the two lines eraſed, one on each ſide, might be made meerly for ornament. Beſides, the face and two ſides only are finiſhed; the back being flat, as if it was deſined to ſtand againſt a wall. The hight of it, which is very near three feet; as likewiſe the form both of the ſtone itſelf, and the plane above mentioned; will appear by the draughts of them taken by Mr. Hoare, which are annexed to this diſcourſe (2). From a

(1) A fine print of this head has been publiſhed by the A. S. L. in their *Vetuſt. Monum. Vol.* 1. *Num.* 34.

(2) See TAB. III. *Fig.* 3.

a careful examination of the whole inſcription, as it appeared to me in the caſt, I have copied it in the draught of the ſtone; and indeavoured to expreſs the ſeveral letters in their proper form and proportional ſize, together with the ligatures, diviſions of the words, and their ſituation in each line, in the moſt exact manner I was capable of doing it. And upon conſidering the whole in this view, I take leave to offer the following reading in words at length, as what appears to me the moſt probable.

Locum religioſum, per inſolentiam erutum,
virtuti et numini Auguſti repurgatum
reddidit Caius Severius Emeritus, centurio,
ſua *pecunia.*

1. That the words LOCVM RELIGIOSVM muſt here ſignify a *burying place*, cannot, I think, be well doubted of; ſince that is the uſual acceptation of them in the Roman laws. Thus *Marcian* ſais: *Religioſum locum unuſquiſque ſua voluntate facit, dum mortuum infert in locum ſuum* (1). The ſame thing is ſaid by *Juſtinian* (2). And in like manner *Ulpian* ſais: *Locum, in quo ſervus ſepultus eſt, religioſum eſſe Ariſto ait* (3). Moreover, as a ſevere puniſhment was inflicted on thoſe, who violated the ſepulchers of the dead (4); ſo for their further ſecurity they

(1) *L.* 6. § 2. *D. de diviſ. rer.*
(2) *Inſtit.* § 9. *eod.*
(3) *L.* 2. *princ. D. de religioſ. et ſumpt. fun.*
(4) *L.* 3. § 7. *D. de ſepul. violat.*

they were consecrated to the *Dii Manes*. And as temples and altars, dedicated by public authority to the *Dii Superi*, were stiled *loca sacra* (1); hence, as *Aggenus Urbicus* observes: *Sacrum proprie Dei est, religiosum hominum* (2).

It is well known, that by the laws of the *Twelve tables* burials were prohibited within the city of *Rome*. The words of the law are these: *Hominem mortuum in urbe ne sepelito, neve urito* (3). The like prohibition was afterwards extended to the provinces, by the emperors *Hadrian* (4), *Antoninus Pius* (5), and others (6). There were indeed some exceptions to this law, in favour of particular persons, which do not come under our present consideration. The *Romans* therefore commonly placed their sepulchers, either near the common roads, or within their own possessions, or in some place alloted for that purpose by authority. Instances of the first sort are to be met with in *Gruter*, and other writers upon this subject. And the reason of it is assigned by *Varro*, who sais: *Monimenta in sepulchris ideo secundum viam sunt, quo praetereunteis admoneant, et se fuisse, et illos esse mortaleis* (7). As to private or family sepulchers, a very full and ample account of them has been given by the late *Roger Gale* esquire, a worthy member

(1) *Festus in voce* Religiosus. *Instit.* § 8. *ubi supra.*
(2) *De controvers. agror. pag.* 61. *edit. Goes.*
(3) *Tab.* x. *L.* 2. *princ.*
(4) *L.* 3. § 5. *D. de sepul. violat.*
(5) *Jul. Capitol. in vit.*
(6) *L.* 12. *C. de religios. et sumpt. fun.*
(7) *De L L. Lib.* v.

ber and treaſurer of this Society, in a diſcourſe writen by him upon that ſubject, and formerly printed in the *Philoſophical Tranſactions* (1), to which I need only refer. The place at *Rome* moſt remarkable for the ſepulchral monuments of illuſtrious perſons, as appears from *Strabo* (2), was the *Campus Martius*, where they were buried by order of the ſenate. But the common burying place alloted for ſlaves, and other mean perſons, is by *Varro* called *Puticulae*; which lay, as he ſais, beyond the mount *Eſquiliae* (3). And to this *Horace* alludes in the following verſes:

Hoc miſerae plebi ſtabat commune ſepulchrum,
Pantolabo ſcurrae, Nomentanoque nepoti (4).

And that the like cuſtom obtained in other parts of the Roman empire, appears from a paſſage in *Aggenus Urbicus*, in which he ſais: *Loca autem, quae ſint publica, videamus.* And then, after ſome others there mentioned, he adds: *Sunt in ſuburbanis loca publica, inopum deſtinata funeribus, quae loca* culinas *appellant* (5). Where under the word *inopum* muſt be included all ſuch perſons, who had no private or family burying places of their own; unleſs ſome particular place was aſſigned them by order of the

(1) *Num.* 441. *pag.* 211.
(2) Διόπερ ἱεροπρεπέστατον νομίσαντες τὸν τόπον τοῦτον, καὶ τὰ τῶν ἐπιφανεστάτων μνήματα ἐνταῦθα κατεσκεύασαν ἀνδρῶν καὶ γυναικῶν. L. v. *pag.* 236. *edit. Caſaub.*
(3) *De L. L. Lib.* IV.
(4) *Lib.* 1. *Sat.* 8. *v.* 10.
(5) *Ubi ſupra, pag.* 60.

the magiſtrates, which was not uncommon, as we find by ſeveral antient inſcriptions (1).

But there was no town, to which this could be more ſuitable than *Bath*, on account of the great number of ſtrangers, who reſorted thither for the benefit of the ſalutiferous ſprings. For as ſome of thoſe, who came from diſtant parts, may be ſuppoſed from time to time to have died there; a public coemetery for the burial of them was highly requiſite. All the Roman inſcriptions hitherto found at or near *Bath* have been of the ſepulchral kind; except that now under conſideration, which however has ſome affinity with them. The reſt are five in number; four of which were firſt publiſhed by *Camden* (2), and ſince by Dr. *Guidott* (3) and others; and the fifth, relating to *Julius Vitalis*, by Dr. *Muſgrave* (4). But of the four in *Camden*, two at leaſt appear to have belonged to perſons, who were not ſettled there, but came from diſtant places, probably upon the account of their health. One of theſe is called *decurio coloniae Glevenſis*, that is, a *ſenator* or *alderman of Glouceſter*, which was then a Roman colony named *Glevum*. The other is ſaid to have been a ſoldier *legionis ſecundae adjutricis*, as thoſe words have been generally read. But, as *Horſley* has remarked, it does not appear, that this legion ever was in *Britain* (5). As moſt of thoſe inſcriptions were found at *Walcote*, a village

(1) See *Sertorii Monument. Patavin.*
(2) *Britann. pag.* 171, 172. *edit.* 1607.
(3) *Diſcourſe of Bathe*, *Chap.* x.
(4) *Julii Vitalis Epitaphium.*
(5) *Britann. Roman. pag.* 326.

village upon the *Fosse road*, about a mile north eaſt from *Bath*; the coemetery is ſuppoſed to have lain at or near that place (1), where, as Dr. *Stukeley* obſerves, there was alſo a Roman camp (2).

2. The next words in the inſcription, PER INSOLENTIAM ERVTVM, are attended with no ſmall difficulty. Some perſons, as I was informed, had taken the firſt letter of the laſt word for a D, which led them to read it *dirutum*; tho there is no appearance of an I after the D, nor indeed room for it upon the ſtone, to ſupport that reading. But on viewing the caſt, the perpendicular ſtroke of that letter ſeemed to me to be carried ſomewhat higher than the other letters of that line, which ſtood before it. And this occaſioned me to ſuſpect, that it might have been deſigned for an abbreviation of the two letters D I, as in another of our Britiſh inſcriptions, where thoſe two letters are ſo combined in the word DILECTI (3). But having conſulted Mr. *Hoare* concerning it, he informs me, that upon a reinſpection he thinks it to be an E; for which reaſon I have writen it *erutum*. However, this will make no difference in the ſenſe; for ſo *Tacitus* uſes *eruere* for *diruere*, when he ſais: *Cerialis poſtero die coloniam Trevirorum ingreſſus eſt, avido milite eruendae civitatis* (4).

When this inſcription was laſt before the Society, a learned gentleman then preſent was pleaſed to query, whether the words *per inſolentiam* might not here ſignify

(1) See *Muſgrave, Jul. Vital. Epitaph. p.* 172.
(2) *Itiner. curioſ. pag.* 140.
(3) *Britann. Roman. York,* XIII.
(4) *Hiſt. Lib.* IV. *cap.* 72.

ignify *through disuse*. In relation to which I would beg leave to observe, that there are indeed some passages in the Roman writers, which may seem to favour such a sense. So *Turpilius* the poet: *Quid mihi vellem, ex insolentia nesciebam* (1). And *Cicero*: *Non superbia, sed istius disputationis insolentia, atque earum rerum inscitia feci* (2). And again: *Moveor loci insolentia, quod tantam causam dico intra domesticos parietes* (3). But in each of these places, and some others, which might be mentioned, the word *insolentia* refers to what never had been used; and not what ceased to be so, as the sense would here require. Besides, it does not seem to suit with the participle *erutum*, with which it is joined. For tho a building may be said to fall down, and come to ruin, through neglect or disuse; yet it is not, I think, usual to say, that it was pulled down or demolished, meerly by length of time, or from want of care to support it. The Latin word commonly used in that case is not *erutum* or *dirutum*, but *collapsum*. And so we find it expressed in another of our British inscriptions: *Templum olim vetustate conlapsum Gaius Julius Pitanus, provinciae praeses, restituit* (4).

This coemetery therefore, as I imagine, had been plundered, and reduced to a ruinous state, by some illegal acts of fraud or violence. Nor does this seem to have been a very uncommon case, notwithstanding

(1) *Apud Nonium in voce* Insolens.
(2) *De orat. Lib.* I. *cap.* 22.
(3) *Pro Dejotar. cap.* 2.
(4) *Britann. Roman. Cumberl.* XXXIV. See likewise *Northumb.* LXXXIX.

ing the many Roman laws yet extant, which were made for their ſecurity (1). For as *Aggenus Urbicus* remarks, ſpeaking of ſuch licentious practices: *Ex his locis, cum ſint ſuburbana, ſine ulla religionis reverentia ſolent privati aliquid uſurpare, atque hortis ſuis applicare* (2). What is here refered to by this writer, is ſufficiently explained by *Julius Paulus* in the following paſſage: *Qui monumento lapidem columnamve ſuſtulerit, ſepulchrum violaſſe videtur* (3). Another inducement for plundering ſepulchers might be the expectation of finding money, or other things of value, depoſited in them. Among the ſeveral prohibitions againſt expenſive funerals, injoined by the laws of the *Twelve tables*, it is ſaid: *Neve aurum addito* (4). This indeed was very ſuitable to the frugality of thoſe antient times. But that it was not conſtantly attended to afterwards, is very evident from the many laws, which were made by ſeveral emperors in ſucceding ages to prevent the burying of money in ſepulchers; which laws would have been unneceſſary, if that practice had not continued. And it has ſometimes happened, that coins, rings, bracelets, and other ſmall ornaments, have been found in Roman urns, both in this and other countries. However, as this practice was ſtrictly ſpeaking illegal, thoſe, who had a property in the ſepulchers, were permited to take away the money found in them; tho it

(1) *D. Lib.* XLVII. *tit.* 12. *de ſepul. violat.* *And C. Lib.* IX. *tit.* 19. *eod.*

(2) *Ubi ſupra,*

(3) *Sentent. recept. Lib.* I. *tit.* 21. § 8.

(4) *Tab.* X. *cap.* 11.

it was judged highly criminal and impious for other perſons, upon any account, to diſturb the aſhes of the dead (1). But I ſhall now proceed with the inſcription.

3. The next words, VIRTVTI ET NVMINI AVGVSTI REPVRGATVM REDDIDIT, acquaint us with the method, which was taken to redreſs this ruinous ſtate of the burying ground. And this, I preſume, by the word *repurgatum* here uſed, was done by removing the rubbiſh; repairing the ſepulchers, which had either been plundered, or in any meaſure demoliſhed; and very probably renewing the fence, which in ſuch places was uſually a wall of ſtones, laid cloſe upon one another without mortar, called by them *maceria*.

But this, it ſeems, was not thought ſufficient for its future ſecurity, without puting it under the immediate protection of the emperor, as its tutelar deity; for nothing leſs than this could, I think, be meant by the words, *virtuti et numini Auguſti reddidit*. The word *numen* in its primary ſenſe ſignifies the *divine power*, as when *Cicero* ſais: *Omnes naturae numini divino parent* (2). And elſewhere he thus addreſſes himſelf to the body of the Roman citizens: *Vos, quorum poteſtas proxime ad deorum numen accedit* (3). But ſoon after, upon the change of the government, when the emperors were revered as deities, their power is likewiſe expreſſed by the term *numen*, as rivaling that of

(1) See *Kirchmann. De funer. Rom. Lib.* III. *cap.* 24.
(2) *De natur. deor. Lib.* I. *cap.* 9.
(3) *Pro Rabir. cap.* 2.

of the gods. And as words, which denote the properties of things, are often made uſe of to expreſs the things themſelves; ſo *numen* is ſometimes uſed in a perſonal ſenſe for a deity. Thus *Horace* complimenting *Auguſtus* upon the divine honours, which were paid him by the Roman people, ſais:

Laribus tuum
Miſcet numen (1).

Nor was this extravagant flattery peculiar to the poets; for, as *Tacitus* informs us: *Rubrio crimini dabatur violatum perjurio numen Auguſti* (2). And in another place he ſais: *Fortunam, et deos, et numen Othonis adeſſe teſtabantur* (3). We find the like uſe of the word alſo in *Valerius Maximus*, who ſpeaking of ſome perſons, who falſely pretended to have ſprung from noble families, thus expreſſes himſelf: *Nec divi quidem Auguſti, etiamnum terras regentis, excellentiſſimum numen intentatum ab hoc injuriae genere extitit* (4). But this occurs moſt frequently in their monumental inſcriptions. So among many other inſtances in *Gruter*, there is one upon an altar at *Narbo*, which has this title:

NVMINI. AVGVSTI. VOTVM
SVSCEPTVM. A. PLEBE. NARBO
NENSIVM. IN PERPETVOM (5).

This

(1) *Carmin. Lib.* IV. 5. 34.
(2) *Annal. Lib.* I. *cap.* 73. Where ſee the *Commentators.*
(3) *Hiſt. Lib.* II. *cap.* 33.
(4) *Lib.* IX. *cap.* 15. § 3.
(5) *Pag.* CCXXIX.

This altar is there ſaid to have been erected in the conſulſhip of *T. Statilius Taurus* and *L. Caſſius Longinus*; that is, in the year of *Rome* 763, as *Pighius* places it (1); or 764, according to cardinal *Noris* (2): which ſhews it to have been done, while *Auguſtus* was yet living. And that the ſame practice continued in the time of the following emperors, is evident from ſeveral of our Britiſh inſcriptions, where the like compliment is paid to them. So we meet with *Numen domini noſtri Auguſti*, *Numen imperatoris*, and *Numen principis optimi* (3), with other variations in the manner of expreſſion. From hence therefore, I think, we may conclude, that by the words *virtuti et numini Auguſti* in this inſcription, not only the prowess or fortitude of the emperor then reigning, but likewiſe the divine power here aſcribed to him, is called in aid for the defence and preſervation of this coemetery. As to the form of the expreſſion here uſed, things were ſaid *diis reddi*, which were offered or conſecrated to them. Thus *Suetonius* ſais: *Cavebatur, ut in poſterum ſimili modo exta Marti redderentur* (4). The horizontal ſtroke over the letter N̄ appears on the ſtone, which is often omited or defaced.

4. The perſon, who conducted this affair, is called CAIVS SEVERIVS; the former of which names occurs very frequently, and the latter ſeveral times, in *Gruter*. But the following word EMERITVS, tho it be

(1) *Annal. Roman. Tom.* III. *p.* 539.
(2) *Cenotaph. Piſan. Diſſert.* I. *pag.* 52.
(3) *Britan. Roman. Northumb.* LXXX. *Cumberl.* LI, LII.
(4) *In vit. Auguſt. cap.* I.

be found alſo as a proper name in *Gruter*, yet more uſually ſignifies a *veteran ſoldier*. And therefore, as the inverted Ↄ, which comes after it, is the common character for CENTVRIO, a military officer in the Roman infantry, who commanded a hundred men; it may poſſibly be doubted, whether by *emeritus centurio* might not here be meant a *ſuperannuated captain*. Beſides, it is well known, that the word *centurio* in other inſcriptions is generally followed by the name of the legion or cohort, to which that officer belonged; which is here wanting. And in *Gruter's* collection mention is made of one *Sergius Terentius, Sergii filius, Aemilianus, centurio emeritus* (1). But however, as in *Gruter* not only the word *emeritus* is put after *centurio*, but the perſon has likewiſe the *cognomen* of *Aemilianus*; I am inclined to think *Emeritus* was the *cognomen* of this *Caius Severius*, and that the legion or cohort might be omited for want of room to inſert it on the ſtone.

5. The ſingle imperfect word in the laſt line is neither by far ſo large, nor ſo deeply cut, as the reſt of the inſcription; which, I preſume, was occaſioned alſo meerly for want of room. In all the copies I have ſeen, it is writen PEG. and Mr. *Hoare* tells me, that the laſt letter is certainly a G. However I cannot much doubt, but it was deſigned for an abbreviation of the word PECVNIA, and had once the pronoun SVA before it. For nothing was more common in public benefactions, than for the perſons, who bore the expence of them, to expreſs it by the words

(1) *Pag.* DLXIII. *num.* 4.

words *ſua pecunia*, *ſua impenſa*, or *ſuo ſumptu*, in the inſcriptions prefixed to them, for the ſake of perpeating their own memory. There are, it ſeems, no traces of the word *ſua* now remaining; but on the contrary, as Mr. *Hoare* ſais, the ſtone is *remarkably ſmooth* in that part: which makes me ſuſpect, there has been an eraſement; tho when, or for what reaſon it was made, would now be in vain to inquire at ſo great a diſtance of time. Nor is this a ſingle inſtance of that kind. I ſhall therefore only obſerve further, that the letter G is no objection againſt the word *pecunia* being there intended. For as the two letters C and G had a great affinity in their ſound, ſo they were often writen one for the other. *Reineſius* has collected a large catalogue of both theſe changes from antient authorities. Among the latter ſort, with which we are concerned, are *Progne* for *Procne*, *legtus* for *lectus*, *Tutigia* for *Tuticia*, and *carruga* for *carruca* (1); to which I ſhall only add *Gaeſari* for *Caeſari*, from another of our Britiſh inſcriptions (2).

6. There are no points in the whole inſcription, except one in the third line, after the word *inſolentiam*; which may be ſuppoſed to have come there rather by chance, than deſign, as it can be of no ſignificancy.

7. After the *Romans* had abandoned this iſland, their monuments of all kinds were left as ſpoils to the inhabitants; who either deſtroyed or converted them to their own uſe, as they pleaſed. And many

(1) *Reineſ. claſs.* 1, *num.* 2. *p.* 4.
(2) *Brit. Rom. Northumb.* LXXVII.

of them have at different times been removed to a confiderable diftance from the places, where at firft they ftood. So two of thofe mentioned by *Camden*, as then fixed in the inner fide of the wall at *Bath*, between the north and weft gates (1), had doubtlefs been conveyed from the coemetery. And it may with equal reafon be fuppofed, that this was likewife brought from thence.

But who the reigning emperor was, at the time this ftone was fet up, no intimation is given in the infcription. Tho, if one may be allowed to conjecture, the form of the letters fuits very well with fome others in the reign of *Severus*. And perhaps no time was more open for fuch licentious practices, as might juftly merit the name *infolentia*, than the loofe reign of *Commodus*; who, altho he was not the immediate predeceffor of *Severus*, yet died but a few months before he came to the empire. Befides, we have two other infcriptions found in *Britain*, addreffed *Numinibus Auguftorum*; both which are thought to relate to *Severus* and his elder fon *Caracalla*, after he was joined with his father in the government (2). Nor can there be any doubt of this, as to one of them at leaft; which is an altar, and has on one fide of it the names of both his fons, *Caracalla* and *Geta*, as confuls that year. So that upon the whole, I can find no other period of time fo probable for fixing the date of this infcription.

G. C. Nov. 2. 1753. John Ward.

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(1) *Britann. pag.* 172. *edit.* 1607.
(2) *Britann. Rom. Northumb.* LXXXVIII. *Yorkfh.* XVIII.

LII.

LII. *A Letter to the right honourable the Earl of* Macclesfield, *President of the* Royal Society, *from Mr.* Benjamin Wilson, *F.R.S. concerning some* electrical Experiments, *made at* Paris.

My Lord, Sept. 18, 1753, Great Queen-str. London.

Read Dec. 6, 1753.

ON the 7th of this month I was at the palace of St. German's near Paris, where I had the satisfaction of seeing Dr. le Monnier's experiment relating to the electricity of the air; of which an account was sent me by Abbè Mazeas, and was read before our honourable Society in December last; and I observed, that though the wind was westerly, and the air moist, yet the suspended wire, at different times, attracted very light bodies, at very small distances; the mean of which distances seem'd to be about $\frac{1}{10}$ of an inch.

Abbè Mazeas informed me, that Dr. le Monnier, some months ago, had read a paper, at a meeting of the Royal Academy of Sciences, in which he told them, that he had great reason to believe the electric matter did not come from the *earth* at all, but from the *air*. Upon my mentioning this to the Doctor, I found him still of the same opinion.

As there was a convenient *apparatus* in his apartment, I proposed making the experiments: for I always thought that the electric matter came from both, but principally from the earth; and that, probably, a difference of 10 to 1 would be perceived, upon making the experiments.

The machine was ſuſpended by ſilk lines, in ſuch a manner, that every part of it was not leſs than two feet diſtant from any non-electric. The lines were dried by a chafing-diſh of fire made with charcoal, as was likewiſe the glaſs globe; and every other precaution was ſtrictly obſerved, that ſeemed neceſſary for making the experiments.

The Doctor appeared to be well verſed in electrical inquiries, and ſhewed great judgment in conducting the whole. He got upon the ſuſpended *apparatus* himſelf, and rubbed the globe with both his hands; whilſt another perſon, who was likewiſe ſuſpended, turned the wheel of the machine. Cloſe to the globe was a ſlender ſlip of lead; at one end of which was faſtened ſome braſs tinſel, to ſerve as a collector of the electric matter. The other end of the lead had a communication with a tin tube, which was ſupported by ſilk lines about a foot in length: and as this tube hung higher than I could reach, another was hooked to it by means of a wire which hung down to a convenient diſtance.

As I ſtood upon the floor, I took hold of this laſt tube, whilſt the glaſs was rubbed, that the *apparatus*, and the perſons on it, might loſe as much of their natural electricity as poſſible under ſuch circumſtances. On removing my hand, and afterwards approaching the tube, ſometimes with my finger, and at other times with a key, we obſerved very ſmall exploſions, which were little more than juſt ſenſible.

I then deſired one of the Doctor's ſervants (who likewiſe ſtood upon the floor) to lay hold of the ſuſpended *apparatus* on which the Doctor was mounted, whilſt the friction of the globe was continued. Immediately

mediatley on approaching the tube as before, with my finger, and then with the key, a very great difference was obſerved; for now the exploſion was very large compared with the former trials. Doctor le Monnier deſired the experiments might be repeated; which was done ſeveral times, and, to all appearance, the differences were the ſame.

He was perfectly ſatisfied that the experiments were fairly made, and that the exploſion was much greater when the *apparatus* communicated with the *earth*, than when it communicated with the *air* only.

As ſeveral gentlemen of the Royal Academy of Sciences in Paris were of opinion that theſe experiments deſerved attention, I thought your lordſhip would not be diſpleaſed, if I did myſelf the honour of communicating them.

I am,

My Lord,

Your Lordſhip's moſt obedient,

and obliged humble ſervant,

Benjamin Wilſon.

For a ſeries of experiments and obſervations where the whole electrical apparatus was ſupported by electrics *per ſe*, ſee *Phil. Tranſ.* Vol. XLIV. p. 713—729—739, 740. See alſo Vol. XLV. p. 93—101 and Vol. XLVII. p. 371, 373. And Mr. Wilſon's *Eſſay*, and *Treatiſe, on Electricity*.

LIII. *Electrical Experiments, with an Attempt to account for their several Phænomena; together with some Observations on* Thunder-Clouds, *by* John Canton, *M. A. and F. R. S.*

Experiment 1.

Read Dec. 6, 1753.

FROM the cieling, or any convenient part of a room, let two cork-balls, each about the bigness of a small pea, be suspended by linen threads of eight or nine inches in length, so as to be in contact with each other. Bring the excited glass tube under the balls, and they will be separated by it, when held at the distance of three or four feet; let it be brought nearer, and they will stand farther apart; intirely withdraw it, and they will immediately come together. This experiment may be made with very small brass balls hung by silver wire; and will succeed as well with sealing-wax made electrical, as with glass.

Experiment 2.

If two cork-balls be suspended by dry silk threads, the excited tube must be brought within eighteen inches before they will repel each other; which they will continue to do, for some time, after the tube is taken away.

As the balls in the first experiment are not insulated, they cannot properly be said to be electrified: but when they hang within the atmosphere of the excited

excited tube, they may attract and condenſe the electrical fluid round about them, and be ſeparated by the repulſion of its particles. It is conjectur'd alſo, that the balls at this time contain leſs than their common ſhare of the electrical fluid, on account of the repelling power of that which ſurrounds them; tho' ſome, perhaps, is continually entering and paſſing thro' the threads. And if that be the caſe, the reaſon is plain, why the balls hung by ſilk, in the ſecond experiment, muſt be in a much more denſe part of the atmoſphere of the tube, before they will repel each other. At the approach of an excited ſtick of wax to the balls, in the firſt experiment, the electrical fire is ſuppoſed to come through the threads into the balls, and be condenſed there, in its paſſage towards the wax: for, according to Mr. *Franklin*, excited glaſs *emits* the electrical fluid, but excited wax *receives* it.

Experiment 3.

Let a tin tube, of four or five feet in length, and about two inches in diameter, be inſulated by ſilk; and from one end of it let the cork-balls be ſuſpended by linen threads. Electrify it, by bringing the excited glaſs tube near the other end, ſo as that the balls may ſtand an inch and an half, or two inches apart: then, at the approach of the excited tube, they will by degrees loſe their repelling power, and come into contact; and as the tube is brought ſtill nearer, they will ſeparate again to as great a diſtance as before: in the return of the tube they will approach each other till they touch, and then repel as at firſt. If the tin-tube be electrified by wax, or the wire of a charg'd

charg'd phial, the balls will be affected in the ſame manner at the approach of excited wax, or the wire of the phial.

Experiment 4.

Electrify the cork-balls as in the laſt experiment by glaſs; and at the approach of an excited ſtick of wax their repulſion will be increaſed. The effect will be the ſame, if the excited glaſs be brought towards them, when they have been electrified by wax.

The bringing the excited glaſs to the end, or edge of the tin-tube, in the third experiment, is ſuppos'd to electrify it poſitively, or to add to the electrical fire it before contained; and therefore ſome will be running off through the balls, and they will repel each other. But at the approach of excited glaſs, which likewiſe *emits* the electrical fluid, the diſcharge of it from the balls will be diminiſh'd; or part will be driven back, by a force acting in a contrary direction; and they will come nearer together. If the tube be held at ſuch a diſtance from the balls, that the exceſs of the denſity of the fluid round about them, above the common quantity in air, be equal to the exceſs of the denſity of that within them, above the common quantity contain'd in cork; their repulſion will be quite deſtroy'd. But if the tube be brought nearer; the fluid without, being more denſe than that within the balls, it will be attracted by them, and they will recede from each other again.

When the apparatus has loſt part of its natural ſhare of this fluid, by the approach of excited wax to one end of it, or is electrified negatively; the electrical fire

fire is attracted and imbib'd by the balls to ſupply the deficiency; and that more plentifully at the approach of excited glaſs, or a body poſitively electrified, than before; whence the diſtance between the balls will be increaſed, as the fluid ſurrounding them is augmented. And in general, whether by the approach or receſs of any body; if the difference between the denſity of the internal and external fluid be increaſed, or diminiſhed; the repulſion of the balls will be increaſed, or diminiſhed, accordingly.

Experiment 5.

When the inſulated tin tube is not electrified, bring the excited glaſs tube towards the middle of it, ſo as to be nearly at right angles with it, and the balls at the end will repel each other; and the more ſo, as the excited tube is brought nearer. When it has been held a few ſeconds, at the diſtance of about ſix inches, withdraw it, and the balls will approach each other till they touch; and then ſeparating again, as the tube is moved farther off, will continue to repel when it is taken quite away. And this repulſion between the balls will be increaſed by the approach of excited glaſs, but diminiſhed by excited wax; juſt as if the apparatus had been electrified by wax, after the manner deſcribed in the third experiment.

Experiment 6.

Inſulate two tin tubes, diſtinguiſhed by *A* and *B*, ſo as to be in a line with each other, and about half an inch apart; and at the remote end of each, let a pair of cork balls be ſuſpended. Towards the middle

of *A*, bring the excited glaſs tube; and holding it a ſhort time, at the diſtance of a few inches, each pair of balls will be obſerved to ſeparate: withdraw the tube, and the balls of *A* will come together, and then repel each other again; but thoſe of *B* will hardly be affected. By the approach of the excited glaſs tube, held under the balls of *A*, their repulſion will be increaſed: but if the tube be brought, in the ſame manner, towards the balls of *B*, their repulſion will be diminiſhed.

In the fifth experiment, the common ſtock of electrical matter in the tin tube, is ſuppoſed to be attenuated about the middle, and to be condenſed at the ends, by the repelling power of the atmoſphere of the excited glaſs tube, when held near it. And perhaps the tin tube may loſe ſome of its natural quantity of the electrical fluid, before it receives any from the glaſs; as that fluid will more readily run off from the ends or edges of it, than enter at the middle: and accordingly, when the glaſs tube is withdrawn, and the fluid is again equally diffuſed through the apparatus, it is found to be electrified negatively: For excited glaſs brought under the balls will increaſe their repulſion.

In the ſixth experiment, part of the fluid driven out of one tin tube enters the other; which is found to be electrified poſitively, by the decreaſing of the repulſion of its balls, at the approach of excited glaſs.

Experiment 7.

Let the tin tube, with a pair of balls at one end, be placed three feet at leaſt from any part of the room, and the air render'd very dry by means of a fire:

fire: electrify the apparatus to a confiderable degree; then touch the tin tube with a finger, or any other conductor, and the balls will, notwithftanding, continue to repel each other; tho' not at fo great a diftance as before.

The air furrounding the apparatus to the diftance of two or three feet, is fuppofed to contain more or lefs of the electrical fire, than its common fhare, as the tin tube is electrified pofitively, or negatively; and when very dry, may not part with its overplus, or have its deficiency fupplied fo fuddenly, as the tin; but may continue to be electrified, after that has been touch'd, for a confiderable time.

Experiment 8.

Having made the Torricellian vacuum about five feet long, after the manner defcribed in the *Philofophical Tranfactions*, Vol. xlvii. p. 370. if the excited tube be brought within a fmall diftance of it, a light will be feen thro' more than half its length: which foon vanifhes, if the tube be not brought nearer; but will appear again, as that is moved farther off. This may be repeated feveral times, without exciting the tube afrefh.

This experiment may be confider'd as a kind of ocular demonftration of the truth of Mr. Franklin's hypothefis; that when the electrical fluid is condenfed on one fide of thin glafs, it will be repelled from the other, if it meets with no refiftance. According to which, at the approach of the excited tube, the fire is fuppofed to be repelled from the infide of the glafs furrounding the vacuum, and to

be carried off thro' the columns of mercury; but, as the tube is withdrawn, the fire is ſuppoſed to return.

Experiment 9.

Let an excited ſtick of wax, of two feet and an half in length, and about an inch in diameter, be held near its middle. Excite the glaſs tube, and draw it over one half of it; then, turning it a little about its axis, let the tube be excited again, and drawn over the ſame half; and let this operation be repeated ſeveral times: then will that half deſtroy the repelling power of balls electrified by glaſs, and the other half will increaſe it.

By this experiment it appears, that wax alſo may be electrified poſitively and negatively. And it is probable, that all bodies whatſoever may have the quantity they contain of the electrical fluid, increaſed, or diminiſhed. The clouds, I have obſerved, by a great number of experiments, to be ſome in a poſitive, and others in a negative ſtate of electricity. For the cork balls, electrified by them, will ſometimes cloſe at the approach of excited glaſs; and at other times be ſeparated to a greater diſtance. And this change I have known to happen five or ſix times in leſs than half an hour; the balls coming together each time, and remaining in contact a few ſeconds, before they repel each other again. It may likewiſe eaſily be diſcover'd, by a charged phial, whether the electrical fire be drawn out of the apparatus by a negative cloud, or forced into it by a poſitive one: and by whichſoever it be electrified, ſhould that cloud either part with its overplus, or have its deficiency ſupplied ſuddenly, the apparatus will loſe its electricity: which

is frequently obſerved to be the caſe, immediately after a flaſh of lightning. Yet when the air is very dry, the apparatus will continue to be electrified for ten minutes, or a quarter of an hour, after the clouds have paſſed the zenith; and ſometimes till they appear more than half-way towards the horizon. Rain, eſpecially when the drops are large, generally brings down the electrical fire: and hail, in ſummer, I believe never fails. When the apparatus was laſt electrified, it was by the fall of thawing ſnow; which happened ſo lately, as on the 12th of November; that being the twenty-ſixth day, and ſixty-firſt time, it has been electrified, ſince it was firſt ſet up; which was about the middle of May. And as Fahrenheit's thermometer was but ſeven degrees above freezing, it is ſuppoſed the winter will not intirely put a ſtop to obſervations of this ſort. At London, no more than two thunder-ſtorms have happened during the whole ſummer: and the apparatus was ſometimes ſo ſtrongly electrified in one of them, that the bells, which have been frequently rung by the clouds, ſo loud as to be heard in every room of the houſe (the doors being open), were ſilenced by the almoſt conſtant ſtream of denſe electrical fire, between each bell and the braſs ball, which would not ſuffer it to ſtrike.

I ſhall conclude this paper, already too long, with the following queries:

1. May not air, ſuddenly rarefied, give electrical fire to, and air ſuddenly condenſed, receive electrical fire from, clouds and vapours paſſing through it?

2. Is not the *aurora borealis*, the flaſhing of electrical fire from poſitive, towards negative clouds at a great

great diſtance, through the upper part of the atmoſphere, where the reſiſtance is leaſt?

LIV. *Extract of a Letter from Profeſſor* Boſe, *of* Wittemberg, *to the Right Honourable* George *Earl of* Macclesfield, *Pr. R. S. with Obſervations thereupon, by Mr.* Wm. Watſon, *F. R. S.*

Wittemberg, Auguſt 1, 1753.

Read Dec. 6, 1753.

IN the beginning of Auguſt 1752, after great and continued rains, many of our rivers overflowed their banks, and covered the neighbouring grounds, more or leſs according to their level, to a conſiderable diſtance: and the quantity of water was ſo great, that in ſome places it was not diſcharged for more than a week. More particularly the river Unſtrut in the territory of the landgrave of Thuringue required a very great time to empty itſelf, not only as that river runs over a large tract of country, but alſo as between Artern and great Jena, where this river joins the Sale, its bed in ſeveral places is very much confined.

When the inundation was abated, it was obſerved from the little city Laucha quite up above Artern, not only upon the fields and meadows, but alſo upon the buſhes and trees, that there was a green and very tough viſcous ſlime, which by the help of a ſtick could be drawn out to two or three ells in length. The ſubſeqnent heat of the ſun dried this matter, and it appeared like wool upon the buſhes; but the fields, when

when ſeen at a diſtance, ſeemed as though they were covered with ſand. This matter had a ſmooth appearance outwards, but within was like a ſheep's-ſkin. Downwards next the ground it had a ſort of wool; and when the whole was waſhed with ſoap, it whitened, and appeared like a clean fleece of white wool. Of this ſubſtance the country-people ſoon made wicks for their lamps, as ſeveral lined their cloaths with it, as they would with furs.

It was further obſerved, that where this ſubſtance was mowed off from the meadows, the graſs under it was quickly dried up; but, where it was not removed, the graſs in the following December was as green and freſh as in the ſpring. —— Thus far Mr. Boſe.

Obſervations.

The vegetable ſubſtance, which, upon the ſpecimen ſent over by the profeſſor to our truly Noble and Learned Preſident, he has intituled "a ſort, "perhaps, of *Alcyonium molle*," is a ſpecies of that genus of plants, which the more modern botaniſts call *byſſus*. And it is of that ſpecies, or a very ſlight variety therefrom, which is called, by the juſtly celebrated Dillenius *, in his *Hiſtoria Muſcorum*, *Byſſus tenerrima viridis velutum referens*. It is alſo mentioned and figured by *Micheli* † in his *Nova Plantarum Genera*, under the title of *Byſſus terreſtris viridis herbacea et molliſſima, filamentis ramoſis et non ramoſis*. This genus of plants, in the order of nature, comes between the moſſes and *fungi*.

* *Hiſtor. Muſcor.* p. 7.
† *Michelii Nov. Plant. Genera*, p. 211. Tab. LXXXIX. Fig. 5.

fungi. The ſpecimen now ſent, being white on one ſide, ariſes from its either being waſhed or bleached by the ſun; for when wet, according to Mr. Boſe, it was green: and this colour is mentioned both by Dillenius and Micheli in their ſeveral denominations.

This vegetable is found in England, as well as in many parts of Europe, in moiſt meadows, covering the ground like a carpet, and ſometimes to a great extent.

We muſt be careful, however, how we connect the ſubſtance in queſtion, and others of the ſame genus with the βύσσος of the antient Greek writers, or the *byſſus* of the Latin. What that ſubſtance was, has been matter of great controverſy. Thus much is certain, that garments made of it were the apparel of the rich. And in the New Teſtament, St. Luke, in the parable of the rich man and Lazarus, ſays of the former, as a mark of his opulence, ἐνεδιδύσκετο πορφύραν καὶ βύσσον; this is tranſlated in our Engliſh verſion, "he was cloathed in purple and fine linen". It is more probable, that the *byſſus* of the ancients was a very fine ſort of cotton: but whoever has the curioſity of examining what has been ſaid upon this ſubject, may conſult Pliny * and Wormius †; but, above all, Bodæus à Stapel §, in his commentary upon Theophraſtus; who has, upon this occaſion, as well as upon a great variety of others, given us an ample teſtimony of his vaſt erudition.

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* *Plinii* lib. xix. c. 1.

† *Muſ.* p. 139.

§ Pag. 425, et ſeq.

LV. *An Account of a Memoir read at the* Royal Academy *of* Sciences *at* Paris, *by M.* de Barros, *a* Portuguese *Gentleman, concerning certain Phænomena observed by him at* Paris, *in the last Transit of* Mercury *over the* Sun: *by* J. Short, *A.M. F.R.S.*

Read Dec. 13, 1753.

THE author says, he made use of an excellent Gregorian reflector of four feet in length, taking in the eye-piece, together with as much of the great tube, as exceeds the focal lengths of the two eye-glasses (*I suppose it should be the two speculums*): that the focus of the great speculum is 33 (*Paris*) inches; that of the small one four inches; the focus of the eye-glass next the eye 18 lines; the focus of the glass furthest from the eye 5 inches; and, lastly, that the combined power of these two glasses is nearly equal to that of a single eye-glass of 3 inches. This telescope, therefore, according to my computation, magnified about 130 times.

That he was placed in the most commodious situation for observing the egress; that his smoaked glass was fixed perpendicular to the axis of his telescope within a close tube; and that he always made use of the same part of this glass.

He took notice, that the interior contact of Mercury's and the Sun's limbs * was very rapid, having observed it with a green-colour'd glass held over the smoaked

* At 10h 18′ 41″.

ſmoaked glaſs: immediately after which, looking thro' the ſmoaked glaſs only, he perceived, that a ſmall thread of light was ſtill viſible between the limbs, before, what he calls, the ſecond contact took place, which was not till four ſeconds after; that the exterior contact appeared ſtationary, or ſeemed to laſt 6 or 7 ſeconds; that having obſerved the total egreſs with the coloured glaſs upon the ſmoaked one, he brought Mercury upon the Sun's limb again, by removing the colour'd glaſs; and that the ſecond total egreſs did not happen till 6 or 7 ſeconds after the firſt. When he obſerved him at the diſtance of about 3 of his diameters from the Sun's limb with both the glaſſes, he remarked, that the ſaid diſtance ſeem'd diminiſhed, and Mercury's diameter increaſed. That the part of the Sun's limb, where Mercury went off, to the extent of ſix degrees of circumference, ſeemed under much the ſame configuration, as the illuminate limb of the Moon about the quadrature, ſomewhat uneven and undulating. The ſame looked alſo redder than the reſt of the diſk. This was about 18 or 20 ſeconds before Mercury diſappeared, and was ſeen thro' the ſmoaked glaſs alone: for when the green glaſs was applied, the appearance in a manner vaniſhed.

The evening before the tranſit, he viewed the Sun with different-colour'd glaſſes, variouſly combined with one another, and with a ſmoaked glaſs; and found, that a green glaſs before the ſmoaked one did beſt: the Sun appearing of a ſilvery hue, like the Moon, and the ſpots and the limb exceedingly well defined.

M. de Barros, having thus deſcribed the particular phænomena, attempts, and indeed very ingeniouſly, to

account

account for them all, from this ſingle ſuppoſition; namely, that the diſks of the Sun, and of Mercury ſeen thereon, are environed with a certain *corona* of light (like that which Sir Iſaac Newton calls the circle of *aberration* or diſſipation in refracting teleſcopes), whereby the apparent diameter of the Sun is inlarged, and that of Mercury contracted.

But, as this gentleman made uſe of a reflecting teleſcope, and as no ſuch circle, from the known principle of reflexion, can take place in ſuch a teleſcope, if well made, as Sir Iſaac has proved long ago; I ſhall not take up the Society's time in purſuing him thro' all his particular ſuppoſitions; but ſhall endeavour to ſhew, that his hypotheſis has really no foundation.

Sir Iſaac, as I before hinted, in ſeveral of his writings, remarks, that the images of all objects ſeen in refracting teleſcopes are ſurrounded with a circle of aberration; which is always leſs, the longer the teleſcopes are. In his optics he tells us, that, to avoid the indiſtinctneſs ariſing from this circle, he would propoſe catadioptric teleſcopes, in which, if the ſpeculums, under limited apertures, be juſtly figured, no ſuch circle of aberration can confuſe the image: but, if the ſpeculums are of a ſpherical figure, with too large apertures, then indeed a circle of aberration will take place; as it alſo will, when the figure deviates from the circular towards the hyperbolic, even under a ſmall aperture; and the ſame thing will happen, if the ſpherical figure be inaccurate.

About three days from the change of the Moon her whole body is viſible: that part of the limb which is directly enlightened by the ſolar rays ap-

pearing to the naked eye, as an arc of a greater circle, than the other, which receives the reflex light from the earth. Look thro' a refracting teleſcope, and you will perceive the apparent difference of theſe circles very much diminiſhed: and if they be view'd with a good reflector, they will be perfectly reduced to an equality, even if meaſured with a micrometer in the focus, as we have often found.

If a reflecting teleſcope, well conſtructed in all reſpects, be directed any conſiderable time to the ſun, ſuch a circle of aberration will be generated, from the little ſpeculum's being heated, and thereby its figure alter'd, from the ſun's rays falling condenſed thereon from the great one; and if it continues long under this circumſtance, the image will be render'd utterly indiſtinct and confuſed.

This we were thoroughly convinced of at the above-mention'd tranſit of Mercury: for the reflector, a very good one, which we uſed, in taking, with the the micrometer, the differences of right aſcenſion and declination between the planet and the ſun's limb, having been a good while expoſed to the direct rays, was found at laſt to give a very indiſtinct image; but was reſtored to its former degree of perfection, by turning it from the ſun, and ſcrewing off the eye-piece, ſo as to admit the cool air into the great tube, whereby the over-heated ſmall ſpeculum ſoon recovered its due temper and figure. The laſt-mentioned effect is ſcarce ſenſible in the leſſer reflectors of ſmall apertures; but in thoſe of large ones it is very conſiderable.

Dr. Bevis, Mr. Canton, and Mr. Bird, who viewed Mercury going off the ſun, with very good reflectors of different lengths, aſſure me, they ſaw him quite

diſtinct

diſtinct and free from any *corona*, or circle of aberration, and the ſun's limb perfectly well defined. For my own part, he appeared to me, thro' a reflector of four feet focus, magnifying about 135 times, as truly defined as I could wiſh to ſee a black circle upon a white ground.

I muſt, however, upon this occaſion, take notice, that, during the whole time of this tranſit of Mercury, the air was perfectly calm with us; but that, in the two laſt tranſits of Mercury over the ſun, *viz.* in the years 1736 and 1743, both the ſun's and Mercury's limbs appeared to me indiſtinct, aud ſurrounded with ſomething like what this gentleman calls a luminous crown, or circle of aberration; tho' I, at both theſe times, made uſe of reflecting teleſcopes, which I had, by former trials, eſteemed good. But it is to be obſerved, that, during both theſe tranſits, there was a conſtant hard gale of wind; and as I had, by other obſervations, formerly found, that the images of the planets, in the night-time, did not appear ſo diſtinct in windy weather, as when it was calm, I therefore imputed the indiſtinctneſs of the ſun's and Mercury's limbs to the air's being agitated by the wind *. And of this we may be made ſenſible by a familiar inſtance:

Suppoſe a veſſel full of water, having any thing lying at the bottom, ſuch as a ſhilling, the water being at reſt; you will then perceive the image of the ſhilling diſtinctly; but if you give any commotion to the water, the image of the ſhilling will then appear indiſtinct and confuſed.

* Since this paper was read, Mr. Short has been informed by M. Le Monnier, the French King's aſtronomer, that, during the laſt tranſit at Paris, they had a hard gale of wind from the N. E.

Somewhat

Somewhat analogous to this is this other appearance: If you look thro' a teleſcope at any of the planets, when the ſtars appear hazy, dim, and languid, you will ſee them diſtinctly: but look at them again, when the ſtars appear moſt bright and ſparking, you will then find their images leſs diſtinct. This may be accounted for by the juſt-mention'd inſtance of the veſſel of water, by ſuppoſing air inſtead of water. And if we conſider the infinite number of heterogeneous particles which continually float in the air, and ſuppoſe theſe to be at reſt, or put into motion, we ſhall find, that it is not at all ſurpriſing, that we ſee the images of objects, placed beyond the medium of air, more or leſs diſtinct.

We are not ſo ſenſible of this indiſtinctneſs, ariſing from the agitation of the air, in refracting teleſcopes, as in reflectors: becauſe the errors of reflexion, cauſed by any irregularity in their figure, or confuſion in the air, are about five or ſix times greater than the ſame errors in refraction; even tho' both teleſcopes magnify the ſame number of times; as has long ago been demonſtrated.

We alſo took notice of M. de Barros's firſt phænomenon; to wit, the ſeeming greater velocity of Mercury when he was near the egreſs: which we thus accounted for. When he was at a conſiderable diſtance from the limb, there being nothing near enough to refer his velocity to, he ſeemed in a manner ſtationary; but being advanced near the ſun's edge, we could refer his motion to that, with eaſe; which thus becoming ſenſible, it might be eſteemed rapid, in compariſon of the former. I have often made the ſame remark on the gradual approach of two lumi-

nous

nous bodies, as the appulſe of the moon's lucid limb to a ſtar or planet.

The expedition with which the author obſerved his ſecond phænomenon, is truly ſomewhat extraordinary; namely, that he ſhould firſt obſerve what he names the final contact; ſecondly, that he ſhould take away his green glaſs; and then, thirdly, that he ſhould be able ſuddenly to alter the conformation of his eye, ſo as to ſee diſtinctly with a much greater influx of light, and then take another obſervation, and all in the ſhort ſpace of four ſeconds! Meſſieurs Mairan and Bouguer take notice of this in their certificate, which follows this memoir; and own, that his obſervations were ſuch, as ſeem to have required the attention of ſeveral perſons.

Upon the whole, we may conclude, that the ſeveral phænomena, obſerved by this gentleman, in the tranſit and egreſs of Mercury, were owing to indiſtinctneſs of viſion, ariſing either from the eye, the teleſcope, or the air; and that this alone may account for them all, without having recourſe to ſuppoſed circles of aberration; which can never poſſibly exiſt in a well-conſtructed reflecting teleſcope.

LVI. *An Explication of an obſcure Paſſage in* Albert Girard's *Commentary upon* Simon Stevin's *Works (Vide* Les Oeuvres Mathem. de Simon Stevin, à Leyde, 1634, p. 169, 170) ; *by Mr.* Simſon, *Profeſſor of Mathematics at the Univerſity of* Glaſgow : *Communicated by the Right Honourable* Philip *Earl* Stanhope.

Read Dec. 20, 1753.

"PUIS que je ſuis entré en la matiere des nombres rationaux, " j'adjouſteray encore deux ou trois particularitez, " non encor par cy devant practiquées, comme d'expliquer les radicaux extremement pres, *&c.*"

The firſt thing Albert Girard gives in this place is a method of expreſſing the ratio of the ſegments of a line cut in extreme and mean proportion, by rational numbers, that converge to the true ratio. For this purpoſe he takes the progreſſion 0, 1, 1, 2, 3, 5, 8, 13, 21, *&c.* every term of which is equal to the ſum of the two terms that precede it: and ſays, any number in this progreſſion has unto the following the ſame ratio [nearly] that any other has to that, which follows it. Thus 5 has to 8 nearly the ſame ratio, that 8 has to 13; conſequently, any 3 numbers next one another as 8, 13, 21, nearly expreſs the ſegments of a line cut in extreme and mean proportion, and the whole line; ſo that 13, 21, 21, (*N. B.* 13 is wrong printed for the ſecond number, inſtead of 21) conſtitute

ſtitute near enough an iſoſceles triangle, having the angle of a pentagon; *i. e.* whoſe angle at the vertex is ſubtended by the ſide of a pentagon in the circle deſcribed about the triangle.

Now this will be plain, if it be ſhewn, that the ſquares of the numbers in this ſeries are alternately leſſer and greater by an unit, than the product of the two numbers next them upon each ſide. Thus, in the four numbers, 5, 8, 13, 21, the ſquare of 8 is an unit leſſer than the product of 5 and 13; but the ſquare of 13 that next follows 8, *viz.* 169, is an unit greater than 8 times 21, or 168; and ſo on conſtantly.

Caſe 1.

If a, b, c, be ſuch numbers, that $\begin{cases} 1.\ a+b=c \\ 2.\ ac=bb+1 \end{cases}$

Then, if d be taken, ſo that $d=b+c$; then ſhall $bd+1=cc$.

Becauſe $d=b+c$; $bd+1$ ſhall be $=bb+bc+1=ac+bc$ [2] which is $=\overline{a+b}\times c=cc$ [1]: *Ergo* $bd+1=cc$.

Caſe 2.

If a, b, c, be ſuch that $\begin{cases} 1.\ a+b=c \\ 2.\ ac+1=bb \end{cases}$

Then, if d be taken, ſo that $d=b+c$; then ſhall $bd=cc+1$.

Becauſe $bd=bb+bc=ac+bc+1$ [2.] $=\overline{a+b}\times c+1$ $=cc+1$ [1.]

Problem.

Having given the number a, in *Caſe* 1. to find b and c, *i. e.* having given a to find b ſuch that $bb+1=(ac=)\ aa+ab$; then is $bb-ab=aa-1$

 and

and therefore $b=\frac{a+\sqrt{5aa-4}}{2}$. Whence, to make b a rational integer number, $5aa-4$ must be a square; which it will be, if $a=1$; and then b will also be 1, and c will be 2: and having continued the series, every number will have the properties mentioned.

The second thing which Albert Girard mentions, is a way of exhibiting a series of rational fractions, that converge to the square root of any number proposed, and that very fast. He tells nothing about the way of forming it, and only gives the two following examples; *viz.*

He says, $\sqrt{2}$ is equal nearly to $\frac{577}{408}$: or, if you would have it nearer, to $\frac{1393}{985}$.

His other example is of $\sqrt{10}$, which, he says, is nearly equal to $3\frac{53353}{328776}$; *i. e.* to $\frac{1039681}{328776}$. And these are the fractions your lordship has turned, at first sight, into continued fractions of the same value *.

The way of making a series of rational fractions, which converge to the square root of any number proposed, in such a manner, that the square of the numerator of any of them being lessened by an unit, or, in some cases, increased by an unit, the remainder, or sum divided by the square of the denominator, shall be exactly equal to the number proposed, depends upon the following propositions:

Prop.

* *N. B.* That the continued fraction here alluded to for expressing the square root of 10 was $\frac{1}{6}\times 19-\cfrac{1}{38-\cfrac{1}{38-\cfrac{1}{38-\cfrac{1}{38}}}}$, &c. *ad infinitum.*

Prop. 1.

Let a be any number propofed, and $\frac{b}{c}$ be fuch a fraction, that $\frac{bb-1}{cc}=a$, i. e. $bb=acc+1$, then, if two other fractions be taken, one of which is $\frac{b}{ac}$, the firft divided by the propofed number a, and the other is $\frac{c}{b}$, the reciprocal of the firft fraction; then the fraction $\frac{bb+acc}{2bc}$, whofe numerator is the fum of the products of the numerators, and of the denominators of the fractions $\frac{b}{c}$ and $\frac{b}{ac}$; and its denominator the fum of the products of the numerators, and of the denominators of the fractions $\frac{b}{c}$ and $\frac{c}{b}$, fhall have the fame property with the fraction $\frac{b}{c}$ i. e. $\frac{\overline{bb+acc}^2-1}{2bc^2}=a$.

Becaufe $bb=acc+1$

$$bb-acc=1,\text{ and fquaring}$$
$$b^4-2ab^2c^2+a^2c^4=1.\quad\text{And adding } 4ab^2c^2$$
$$b^4+2ab^2c^2+a^2c^4=4ab^2c^2+1.$$

Whence $\frac{\overline{b^2+acc}^2-1}{2bc^2}=a$.

Prop. 2.

If $\frac{b}{c}$ be fuch a fraction, that $\frac{bb+1}{cc}=a$, i. e. $bb+1=acc$, all other things remaining as in *Prop.* 1.; then fhall the fraction $\frac{bb+acc}{2bc}$, formed as there defcribed, be fuch, that $\frac{\overline{bb+acc}^2-1}{2bc^2}=a$,

Beause $bb+1=acc$, then $acc-bb=1$; and squaring
$b^4-2ab^2c^2+a^2c^4=1$.

Whence, as in the foregoing, it will follow, that
$\frac{\overline{bb+acc}^2-1}{\overline{2bc}^2}=a$.

Prop. 3.

Let the fraction $\frac{b}{c}$ be such, that $\frac{bb-1}{cc}=a$, i. e. $bb=acc+1$; also let $\frac{d}{e}$ be another fraction, having the same property with $\frac{b}{c}$, *i. e.* such, that $dd=aee+1$. Then, if from the fraction $\frac{d}{e}$, and the two others mentioned in *Prop.* 1. *viz.* $\frac{b}{ac}$, and $\frac{c}{b}$, a new fraction be formed, in the same manner as the fraction $\frac{bb+acc}{2bc}$ was formed from $\frac{b}{c}$, and the same two $\frac{b}{ac}$ and $\frac{c}{b}$, which fraction will be $\frac{bd+ace}{cd+be}$; this new fraction shall have the same property with the other two $\frac{b}{c}$ and $\frac{d}{e}$, *i.e.* $\frac{\overline{bd+ace}^2-1}{\overline{cd+be}^2}=a$.

1. $bb=acc+1$
2. $dd=aee+1$
3. $ac^2d^2=a^2c^2e^2+ac^2$ [2.]
4. $b^2d^2=ab^2e^2+b^2$ [2.]
5. $b^2d^2=ab^2e^2+ac^2+1$ [4, 1.]
6. $b^2d^2+a^2c^2e^2=ab^2e^2+a^2c^2e^2+ac^2+1$ [5.]
7. $b^2d^2+a^2c^2e^2=ac^2d^2+ab^2e^2+1$ [6. 3]
8. $b^2d^2+2abcde+a^2c^2e^2=ac^2d^2+2abcde+ab^2e^2+1$ [7].

i. e. $\overline{bd+ace}^2 = a \times \overline{cd+be}^2 + 1.$

9. $\dfrac{\overline{bd+ace}^2 - 1}{\overline{cd+be}^2} = a.$

Prop. 4.

The same things being supposed as in *Prop.* 3. except that bb, instead of being equal to $acc+1$, as there, is equal to $acc-1$, or $bb+1=acc$; it will follow, by the like steps as in *Proposition* 3. that $\dfrac{\overline{bd+ace}^2 + 1}{\overline{cd+be}^2} = a.$

Prop. 5.

If likewise d^2 be equal to $aee-1$, as well as $b^2 = acc-1$, all other things remaining as in *Proposition* 3. then shall $\overline{bd+ace}^2 = a \times \overline{cd+be}^2 + 1$, *i. e.* $\dfrac{\overline{bd+ace}^2 - 1}{\overline{cd+be}^2} = a.$

$$\begin{cases} 1.\ b^2+1=acc \\ 2.\ d^2+1=aee \end{cases}$$

3. $b^2d^2+b^2=ab^2e^2$ [2.]
4. $ac^2d^2+ac^2=a^2c^2e^2$ [2.]
5. $b^2d^2+ac^2=ab^2e^2+1$ [3, 1.]
6. $b^2d^2+ac^2+ac^2d^2=ac^2d^2+a^2be^2+1$ [5.]
7. $b^2d^2+a^2c^2e^2=ac^2d^2+ab^2e^2+1$ [6.4.]
8. $b^2d^2+2abcde+a^2c^2e^2=ac^2d^2+2abcde+ab^2e^2+1$ [7.]

i. e. $\overline{bd+ace}^2 = a \times \overline{cd+be}^2 + 1.$

9. $\dfrac{\overline{bd+ace}^2 - 1}{\overline{cd+be}^2} = a.$

Prop. 6.

But if $b^2 = acc + 1$, and $d^2 = aee - 1$, all other things remaining as in *Prop.* 3. Then shall $\overline{bd+ace}^2 + 1 = a \times \overline{cd+be}^2$. i. e. $\frac{\overline{bd+ace}^2 + 1}{\overline{cd+be}^2} = a$. which may be shewn, as the rest were.

Now, let a be any number proposed, and let the fraction $\frac{b}{c}$ be such, that either $\frac{bb-1}{cc} = a$, or $\frac{bb+1}{cc} = a$, and take the fractions $\frac{b}{ac}$ and $\frac{c}{b}$, before described; then the series of fractions converging to $\sqrt{a}$ will be as follows:

$\left.\begin{matrix}\frac{c}{b}, \frac{b}{ac}\end{matrix}\right\} \frac{b}{c} =$ the first term of the series.

$\frac{bb+ace}{2bc} = \frac{d}{e}$ the second term.

$\frac{bd+acc}{cd+be} = \frac{f}{g}$ the third term.

$\frac{bf+acg}{cf+bg} = \frac{h}{k}$ the fourth term.

&c. in infin.

Every term is formed from the preceding; and the 2 fractions $\frac{b}{ac}$ and $\frac{c}{b}$, in the same manner as the second from the first, and these fractions.

And from the foregoing propositions it follows,

1. That if $\frac{bb-1}{cc} = a$, then every fraction of the series shall be such.

That if from the square of its numerator be taken an unit, the remainder, divided by the square of its denominator, shall be equal to a.

For, by *Prop.* 1. the fraction $\frac{d}{e}$ shall be such; and by *Prop.* 3. the next fraction $\frac{f}{g}$ shall likewise be such; and so all the following terms.

Example.

Example.

Let $a=2$; then the firſt fraction, *i. e.* that in the ſmalleſt numbers, $\frac{b}{c}$, that makes $\frac{bb-1}{cc}=2$, is when $b=3$, and $c=2$; ſo that

$\frac{c}{b}$. $\frac{b}{ac}$ $\Big\}$ $\frac{b}{c}$

are And the terms following the firſt $\frac{3}{2}$

$\frac{2}{3}$. $\frac{3}{4}$ $\Big\}$ $\frac{3}{2}$ are $\frac{17}{12}$. $\frac{99}{70}$. $\frac{577}{408}$. $\frac{3363}{2378}$. &c.

2. But if $\frac{bb+1}{cc}=a$, *i. e.* if the firſt fraction $\frac{b}{c}$ of the ſeries have the ſquare of its numerator an unit leſs than acc, the multiple of the ſquare of its denominator by the number a; the ſecond term ſhall have the ſquare of its numerator an unit greater than the ſaid multiple of the ſquare of its denominator; and the third term ſhall have the ſaid ſquare an unit leſſer, and ſo on alternately.

For, by *Prop.* 2. the ſecond term $\frac{d}{e}$ ſhall be ſuch, that $\frac{dd-1}{ee}=a$: and therefore, by *Prop.* 4. the third term $\frac{f}{g}$ ſhall be ſuch, that $\frac{ff+1}{cc}=a$. And by *Prop.* 5. it follows, that the next term $\frac{h}{k}$ ſhall be ſuch, that $\frac{hh-1}{kk}=a$; and ſo on alternately, by *Prop.* 4. and 5.

Example.

Let $a=2$; then the firſt fraction $\frac{b}{c}$ that makes $\frac{bb+1}{cc}=2$, is when $b=1$, and $c=1$. So that

$\left.\begin{matrix}\frac{c}{b}. & \frac{ac}{b}\end{matrix}\right\}\begin{matrix}b\\c\end{matrix}$

And the following terms

$\left.\begin{matrix}\text{are}\\ \frac{1}{1}. \frac{1}{2}\end{matrix}\right\} \frac{1}{1}$ are $\frac{3}{2}. \frac{7}{5}. \frac{17}{12}. \frac{41}{29}. \frac{99}{70}$ &c.

But if a be 13, then the first fraction will be $\frac{18}{5}$

$\left.\frac{5}{18}. \frac{18}{65}\right\}$ $\frac{18}{5}. \frac{649}{180}. \frac{23382}{6485}$ &c.

3. But if the fraction $\frac{b}{c}$ be such, that $\frac{bb-1}{cc}=a$, and if the fractions $\frac{b}{ac}$, $\frac{c}{b}$, be taken, from which the series is to be formed, as has been described; then, if the first fraction of the series be made not $\frac{b}{c}$, but some fraction $\frac{d}{e}$, such that $\frac{dd+1}{ee}=a$; then shall every term of the series be such as the fraction $\frac{d}{e}$, *i. e.* the square of the numerator being increased by an unit, and the sum divided by the square of the denominator, the quotient shall be equal to a.

For, since $bb=acc+1$, and $dd=aee-1$, by *Prop.* 6. it follows, that the next term $\frac{f}{g}$ shall be such, that $\frac{ff+1}{gg}=a$; and so on for every term.

Example.

Let $a=2\ \frac{b}{c}=\frac{3}{2}$; then will $\frac{b}{ac}=\frac{3}{4}$, and $\frac{c}{b}=\frac{2}{3}$, and let $\frac{d}{e}=\frac{1}{1}$; then

$\left.\frac{c}{b}. \frac{b}{ac}\right\} \frac{d}{e}$

are

$2\ 3\}\frac{1}{}$

And the other terms

are $\frac{7}{5}$. $\frac{41}{29}$. $\frac{239}{169}$. $\frac{1393}{985}$. &c.

To find $\frac{b}{c}$ ſuch as makes $bb - 1 - acc$, *i. e.* $acc + 1 = bb$, recourſe muſt be had to Lord Brouncker's method in Dr. Wallis's *Commercium Epiſtolicum.*

LVII. *Obſervations upon the* Electricity *of the* Air, *made at the* Chateau de Maintenon, *during the Months of* June, July, *and* October, 1753; *being Part of a Letter from the Abbé* Mazeas, *F.R.S. to the Rev.* Stephen Hales, *D.D. F.R.S. Tranſlated from the* French *by* James Parſons, *M.D. F.R.S.*

SIR,

Read Dec. 20, 1753.

BEING aſſured, that the electricity of the atmoſphere would yet afford means of entertaining you, I ſpent part of this ſummer in obſerving what nature preſented me upon ſo important a ſubject.

On the 14th of June I accompanied the Marechal de Noailles to his caſtle of Maintenon. At my arrival, I ſet up an apparatus, which conſiſted of an iron wire 370 feet long, raiſed to 90 feet above the horizon. It came down from a very high room in the caſtle, where it was faſtened to a ſilken cord ſix

feet long, and was carried from thence to the ſteeple of the town; where it was likewiſe faſten'd to another ſilken cord of eight feet long, and ſhelter'd from rain: and a large key was ſuſpended by the end of this wire, in order to receive the electrical fluid.

Obſervation 1.

From the 17th of June, the time of beginning my experiments, the electricity of the air was ſenſibly felt every day, from ſun-riſe, to 7 or 8 in the evening; except in moiſt weather, when I could perceive no ſigns of electricity. In dry weather, the wire attracted minute bodies, at no greater diſtance than three or four lines. I repeated the experiment carefully every day, and conſtantly obſerved, that, in weather void of ſtorms, the electricity of a piece of ſealing-wax of two inches long was above twice as ſtrong as that of the air. This obſervation inclines me to conclude, that, in weather of equal drineſs, the electricity of the air is always equal.

Obſ. 2.

When I graſped the wire cloſely in my hand, the electricity ceaſed inſtantly, and did not recover till three or four minutes after: whereas, during a ſtorm, we could deprive the wire of its electricity but a moment; for it immediately return'd with the ſame vigour. Whence it appears, that the common electricity of the air has but a ſlow motion.

Obſ. 3.

I endeavoured to increaſe the electricity of my wire, by the addition of a ſecond, which communicated with

with an electrical magazine, composed of pieces of iron, tin plates, gilt paper, and such-like, sustained by silken cords; and I observed, 1. That the electrical fluid did not even then act with any more strength upon minute bodies presented to the wire. 2. That, in depriving this magazine of its electricity, it seem'd to return the more slowly, the more considerable the magazine was; whereas the contrary happens during a storm. This slowness, with which the common electricity of the air is propagated, makes me despair of finding means capable of rendering its motion sensible.

Obs. 4.

It does not appear to me, that hurricanes and tempests increase the electricity of the air, when they are not accompanied with thunder: for, during three days of a very violent continual wind in the month of July, we were obliged to put the dust within four or five lines of the conductor, before any sensible attraction could be perceived. The direction of the winds, whether east, west, north, or south, does not make any sensible alteration in the electricity of the air, except when they are moist. In the most dry nights of this summer, I could observe no signs of electricity in the air; but it returned in the morning, as I have said, when the sun began to appear above the horizon, and vanished again in the evening, about half an hour after sun-set. The strongest common electricity of the atmosphere, during this summer, was perceived in the month of July, on a very dry day, the heavens being very clear, and the sun extremely hot. The distance of ten or twelve lines

was then ſufficient for the approach of the duſt to the conductor, in order to ſee the particles riſe in a vertical direction, like the filings of iron on the application of a magnet.

Obſ. 5.

On the 27th of June, at 2 afternoon, I perceived ſome ſtormy clouds riſing above the horizon, and immediately went up to my apparatus; and having applied the duſt to the key, it was attracted with a force, which increaſed in proportion as the clouds reached the zenith. When they had come nearly over the wire, the duſt was ſo impetuouſly repell'd, as to be intirely ſcatter'd from the paper. I drew conſiderable ſparks from it, altho' there was neither thunder nor lightning. Theſe ſparks were of a very lively red colour, when I attracted them with my finger: they were white and ſmaller, when I uſed a wire hafted in a glaſs tube: they were bluiſh, and much extended, when attracted by ſpirit of wine in a ſilver ſpoon.

Obſ. 6.

I applied a piece of reſin to the conductor, but could draw no ſparks from it: however, all, who were preſent, heard a noiſe like that of hairs when burnt. It was the ſame with ſealing-wax, woolen-cloth, linen, &c. Then I took a quickſilver'd glaſs, and applied to the clean ſide a piece of wire of ſix inches long, whilſt the other end was put to the conductor; by which I drew a multitude of ſmall whitiſh ſparks, which ſoon ceaſed, but were ſucceeded by a

noiſe

noiſe, like that which happen'd upon applying the reſin to the conductor.

I imagine, that this noiſe proceeds from the violent efforts made by the electrical fluid, in order to penetrate into the wire. Since that fluid has only a determined ſpace to occupy in it, it is natural to think, that when that ſpace is fill'd, the fluid ought to produce a noiſe like that, which is heard, when the bottle in the Leyden experiment is greatly charged.

When I applied the end of the wire to the ſilver'd ſurface of the glaſs, whilſt the other end touched the conductor, the quickſilver affected me ſo ſtrongly, that, notwithſtanding my being ſo much accuſtomed to ſuffer theſe electrical ſhocks, I was not able to bear this.

From hence I conclude, that the beſt method of increaſing the electrical power is to make it fall upon ſome metalline ſurface, intimately connected with a ſurface, that is an electric *per ſe*. And in order to bring reaſon and experience together, this is the manner, in which I think the power augmented:

When the electrical fluid is puſhed with rapidity along the conductor (as it happens during a thunder-ſtorm), it ought to be inſtantly diffuſed over the ſurface of the quickſilver. This fluid communicating with the glaſs more eaſily than with the air (which we ſhall prove by-and-by), it ought partly to enter into the ſubſtance of the glaſs: for the difficulty, which it finds in paſſing through its pores, gives it time to accumulate, and conſequently to ſhock bodies applied to it with the greater power. But this method, which ſeems ſo convenient to increaſe the force of the

the electrical fluid in a ſtorm, never ſucceeded for the common electricity of the air. I always found it nearly extinct, never exerting itſelf beyond a certain ſphere of action, and that very inconſiderable.

Obſ. 7.

When the ſtormy clouds were in the zenith of my wire, I obſerved, that the electricity was increaſed to ſo high a point, that the ſilken thread attracted light bodies at the diſtance of ſeven or eight inches. This cord was ſix feet long, and in the firſt foot the electricity was nearly as ſtrong as in the wire, but from thence it diminiſhed in the reſt of the length. I ſubſtituted a glaſs tube to the ſilken cord, and obſerved the ſame phænomenon, with this difference, that the electrical fluid penetrated it with greater difficulty.

Obſ. 8.

The ſtormy clouds, which I mentioned before, remained about two hours above the horizon, without either thunder or lightning; nor did a very heavy rain diminiſh the electricity, except about the end, when the clouds began to be diſſipated; and for that time I left my apparatus.

About ſix o' clock in the evening I was told, that there were ſigns of a new ſtorm in the air: I went up, and while I was preparing matters, a young man of the town, thirty-five years old, ſubject to an epilepſy, was among the ſpectators. The ſmall time, which the ſtorm laſted, was not enough to make many trials in; and the following was what I moſt attended to:

I drew

I drew ſparks upon the epileptic perſon, who was preſent, from the firſt thunder-clap. At firſt he bore them; but in two or three minutes I perceived his countenance change; and, for fear that any accident ſhould happen to him, I begg'd he would retire. He was no ſooner returned home, than his ſenſes fail'd him, and he was ſeized with a moſt violent fit. His convulſions were taken off with ſpirit of hartſhorn; but his reaſon did not return in an hour and half. He went up-and-down-ſtairs, like one who walks in his ſleep, without ſpeaking or knowing any perſon, ſettling his papers, taking ſnuff, and offering chairs to all that came in. When he was ſpoken to, he pronounced inarticulate words, which had no connexion of ſenſe.

When this poor man recovered his reaſon, he fell into another fit. His friends told me, that he was more affected with this diſtemper when it thunder'd, than at any other time; and that if at any time it happen'd, which it rarely did, that he then eſcaped, his eyes, his countenance, and the confuſion of his expreſſions, ſufficiently demonſtrated the weakneſs of his reaſon.

The next day I learned from himſelf, that the fear of thunder was not the cauſe of his diſeaſe; but that, however, he found a fatal connexion between phænomenon and that diſtemper, with which Providence was pleaſed to afflict him. He added, that when the fit ſeized him, he perceived a vapour riſing in his breaſt, with ſo much rapidity, that he loſt all his ſenſes, before he could call for help.

Such are the obſervations, Sir, which my moments of leiſure ſuffer'd me to make: I ſhould have been

very glad, if the matter were more thoroughly examined, and my reſearches more worthy of being preſented to you: but you are better acquainted with nature than any body, and you well know how difficult it is to follow her in her operations. As to the reſt, I ſhall think myſelf well rewarded for my trouble, if ſhe more frequently gives me the pleaſure of amuſing you.

I have the honour to be, with a ſincere and reſpectful attachment,

S I R,

Your moſt humble and

obedient ſervant,

W. Mazeas.

END of PART I.